SCIENCE

NEW SERIES. VOL. VI. No. 133. FRIDAY, JULY 16, 1897. SINGLE COPIES, 15 CTS. ANNUAL SUBSCRIPTION, $5.00.

SCIENCE

FRIDAY, JULY 16, 1897.

CONTENTS:

MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

ON THE THEORY OF ORGANIC VARIATION.*

As the evolution question becomes more and more deeply examined the particular phenomena described under the terms variation and heredity are concentrating much closer observation and thought. The whole philosophy of the matter seems to turn upon the interpretation of these phenomena. In this discussion biologists and those who are engaged in adjusting biological theories to the systems of human thought appear to be resting on the assumption that the great result of the speculations of the last fifty years has been the furnishing of a rational explanation of the so-called natural causes of variation of organisms in their morphological and physiological aspects. This assumption appears to be associated with another, which in some sense is its antithesis, *i. e.*, that those organic phenomena which recur in relatively uniform cycles in successive generations of organisms are fundamental, are the expressions of the intrinsic nature of organic matter, and thus lie beyond the immediate investigation of science. According to this view, heredity (*a*) is not caused, but is a primary law of all organisms; variation (*b*) is a departure from the strict operation of the law of recurrence in generation; and thus external environment (*c*), or the general conditions of being in which organisms exist, is effective in its interaction with the intrinsic energies of the organism in diverting or modifying the natural expression of those energies, to the causing of that diversity and heterogeneity of form and operation which we see about us. This is, as I understand it, a fair expression of the general attitude of thinking men toward the problem in question. This position has received little consideration because it has

*An address delivered before the Philosophical Club of Yale College, April 1, 1897.

been taken for granted as a fundamental truth. It may be tersely expressed in the following form:

1. Heredity is a fundamental law of all organisms.

2. Variation is an acquired law of organism, and is determined by the interaction of the heterogenous environment upon the otherwise uniformly operating organic matter.

It is the validity of these two propositions that I would call in question and discuss. Is heredity acquired? When the attempt is made to state with precision what takes place in the phenomena of organic evolution the question arises: What is the relation which variation bears to heredity as a factor in the case? Which is fundamental; which is acquired?

In order to make the real point of the case clear, let us take an analogy from the field of physics. In astronomical phenomena the planetary bodies are observed to be in rotary motion about the sun and about their own axes; is the rotary motion fundamental or acquired? The first law of Newton is this: "Every body continues in its state of rest, or of uniform motion in a straight line, except in so far as it is compelled by force to change that state." And force is defined as "whatever changes the state of rest or uniform motion of a body." According to these accepted laws of mechanics, rotary motion must be regarded as secondary to direct motion or translation. In order to explain rotary motion, the law of gravity is brought in, and the circular motion is defined as the resultant of the motion of translation adjusted to the gravitation of the masses of matter in motion.

From this analogy the real nature of the problem may be inferred when we ask what is the relation which heredity bears to variation in the field of biology? If it be a fundamental law of organisms to repeat themselves in cycles of generation, to adopt the analogy of mechanics and physics, it is essential to postulate some force to account for any deviation from such hereditary cycles. If, on the other hand, variation be the primary law of organism, the postulated force is required to account for the repetition: such a force would operate first in checking the variation. It will thus be seen that the explanation of the phenomena of life will be greatly modified according as one or other of these hypotheses be assumed to be true.

If we look back over the history of opinions in natural history we discover that a century ago the whole philosophy of organisms was dominated by the Cuvierian notion that species are immutable; *i. e.*, that the cycles of phenomena presented in the development of an individual organism in the passage from the embryo to the adult stages are the same for all members of a species; that this uniformity in expression of development by the individual is the mark or distinguishing characteristic of each species, and therefore that variations, or departures from this fixed law, are accidents due to the disturbing effects of outward environment upon the individual, and cannot be transmitted to offspring by the ordinary laws of heredity. It was this conception of the immutability of species which made 'special creation' of species seem to be a rational theory, and it was the calling in question of the immutability of species which was supposed to limit the capacity of creative force in the universe.

Lamarck advanced the theory that species are not fixed, but are mutable; and he was supposed to be attacking the very foundations of natural history; he was laughed at and his theory was, for the time, silenced by the weight of authority and the common opinion of naturalists. One of the strongest arguments used against his theory was the very fact of species as

they were then known to science. If they are mutable, it was argued, how would it be possible to separate so many which are known as perfectly distinct and not even capable of crossing so as to mix one with another. Sterility was seen to be an impassable barrier distinguishing one species from another, and, as the individuals of one generation are to be accounted for only as descendants of a previous one, how would it be possible to make this barrier on the theory that the law of each species is not fixed and immutable? But although the theory of mutability of species was thought to be absurd a century ago, and was supposed to contradict the fundamental principles of natural history, the idea of specific mutability has now become an established truth in our philosophy of organisms, and variability, or the power of the organism to *divert* from the current paths of development of its ancestors, has become the important factor in evolution. We find, in fact, Bateson, in his elaborate treatise on variation,* saying: "Variation whatever may be its cause, and however it may be limited, is the essential phenomenon of evolution; variation, in fact, is evolution" (p. 6).

When we follow up the history a step further we discover that the theory of mutability of species is built directly upon the Cuvierian philosophy, but it is by breaking down the distinction between varieties and species as originally understood. Lamarck and Darwin both accepted the old conception of the normal or fundamental uniformity of the processes of generation, but, recognizing the fact of departure from this uniformity, assumed that the variation is due to the active adjustment by the organism of its structure to changed conditions of environment (Lamarck); or, the variations being spontaneous or accidental, they are preserved and transmitted in generation from parent to off-spring (Darwin).

*Materials for the study of variation treated with special regard to discontinuity in the origin of species, by Wm. Bateson. 1894.

Both schools and, so far as I have observed, the great majority of all those who discuss these problems, have started with the assumption that the normal province of what is called by the general name of reproduction is the cyclical repetition of phenomena expressed in the ancestors, *i. e.*, that the phenomena will be alike unless some cause can be discovered for their dissimilarity. Hence to discover the cause of differences has been the chief purpose of observers and speculators.

Darwin's ideas regarding the nature of variability in organism are clearly set forth in his 'Origin of Species.' In the first chapter on 'Variation under Domestication,' under the general title 'Causes of Variability,' we find this significant sentence: "It seems pretty clear that organic beings must be exposed during several generations to the new conditions of life to cause any appreciable amount of variation, and that when the organization has once begun to vary it generally continues to vary for generations" (p. 14). A few sentences further on are these words: "It has been disputed at what period of life the causes of variability act," and "I am strongly inclined to suspect that the most frequent cause of variability may be attributed to the male and female reproductive elements having been affected prior to the act of conception," and again: "When any deviation of structure often appears, and we see it in the father and child, we cannot tell whether it may not be due to the same cause having acted on both" (p. 19). Again in the chapter on 'Laws of Variation:' "Nevertheless, we can, here and there, dimly catch a faint ray of light, and we may feel sure that there must be some cause for each deviation of structure, however slight"

(p. 121). And the closing sentence of this chapter: "Whatever the cause may be of each slight difference in the offspring from their parents, and a cause for each must exist, it is the steady accumulation, through natural selection, of such differences, when beneficial to the individual, that gives rise to all the more important modifications of structure by which the innumerable beings on the face of this earth are enabled to struggle with each other, and the best adapted to survive" (p. 153).

I quote thus freely from this standard author and classic treatise on evolution, in order that we may see what the assumption in the case actually is, and also to show that it is a fundamental assumption at the very foundation of the current philosophy of evolution. The quotations are sufficient to show that it was assumed that particular variations are particularly caused, *i. e.*, while variability may be regarded as the possibility of varying, or the latent capability to vary, each variety was, by Darwin, considered to be caused by a special something with which the organism comes into relation and which did not operate upon its ancestors.

I do not propose here to discuss the metaphysical question as to whether an organism may or may not be said to possess powers or potencies, properties or capabilities, or whether it is necessary or not to assume that an organism is capable of varying before it does vary. But in this paper attention is called to the relation which a certain class of biological phenomena bear to another class of biological phenomena, and, so far as it may be possible to confine one's attention to them, these phenomena alone will be considered. From this point of view variation is a deviation, in the order of sequence, of one series of phenomena from some other order of sequence with which it is compared. In the case of organisms the latter series of phenomena is that which the parent form (A) exhibits in the course of its growth from the ovum to maturity. The case we compare with it is the series of phenomena expressed by an offspring (B) in passing from the ovum to the mature stage. A variation occurs whenever, in any particular stage of the series, B varies or deviates from the series A. Supposing such a deviation to take place the morphological character (v) expressed in the structure of the organism (B) is often and may properly be called a variation. The whole organism B, with its added character (v), is often spoken of in biology as a variety of A, and all descendants of A exhibiting the variation (v) are said to be of this variety (Bv). As I understand the Darwinian doctrine (and I believe this is the generally accepted doctrine on this point) it is assumed that except for some special cause acting upon the organism A, or its ancestors for each particular variation (v) of this kind, there would appear no deviation in B, there would arise no variety Bv. Let us be careful not to raise the question whether the organism could vary or could not; the question is purely regarding the order of the phenomena. It is a question of science as to whether the variation takes place on account of some cause (I use the word used by Darwin and suppose we may infer that he means some interference with the course of phenomena taking place in the developing organism), and I raise the question: Have we any evidence to support the opinion that variations would not occur except for some such interference with the normal processes of development exhibited by the growing individual?

If we examine Professor Cope's 'Primary Factors of Organic Evolution,' standing, as it does, for the most extreme of the Neo-Lamarckian school of naturalists as contrasted with the Darwinian, we find a similar assumption on this point. Cope divides

his treatise into three parts, which he entitles: I. The Nature of Variation; II. The Causes of Variation; III. The Inheritance of Variation. He begins the Part III. by saying that he proposes to cite "examples of the direct modifying effect of external influences on the character of individual animals and plants. These influences fall naturally into two classes, viz., the physico-chemical (molecular) and the mechanical (molar). The modifications so presented are supposed to be the result of the action of the causes in question, continued throughout geological time" (p. 225). A few of the examples cited are the conversion of *Artemia salina*, a salt-water Crustacean into a *Branchinecta*, a genus accustomed to fresh-water habits; the production of colors in Lepidopterous pupæ; light and feeding affecting the color of fish; the case of the blindness of cave animals; Dr. Dall's theory of the origin of plaits in the Gastropoda; the moulding the shape of the articulation of bones in accordance with the dominant strain put upon them; the mechanical origin of dental types in Vertebrates.

No question is here raised as to the reality of the observed phenomena; the association of particular modifications of organic structure with change in the conditions of environment to which the organisms are subjected is not disputed. But the specific question raised is this: Does environment in general, or do the external influences of a chemico-physical or mechanical nature (to use Cope's phrases), exert an influence over growing organisms to induce them *to depart from* the order of phenomena of their ancestors, or do these influences or forces produce the opposite effect of *controlling and limiting* variation? From the quotations it will be evident that the theory is clearly expressed in the writing of the two prominent schools of evolution of to-day that *these external influences do, either directly or indirectly, produce the variations.*

Several years ago my studies led me to doubt the validity of this view, and a careful study of the order of sequence shown in the succession of species in geological time has confirmed this opinion. My friend and former neighbor, Professor Bailey, of Cornell, a pupil and ardent disciple of Asa Gray, has been led to the same conclusion from the study of plants, and he has supported and given botanical evidence for the validity of the theory in this book on 'The survival of the unlike' (Macmillan, 1896, see p. 21, 22). I also presented some evidence of paleontological nature which seems to support the view ('Geological Biology,' 1894'). The present paper is intended to consider the philosophical line of reasoning upon which the theory rests.

The commonly held conception seems to be that variation, and consequently the essential essence of evolution, is some kind of modification of ordinary generation. So much is this true that in most minds and in standard treatises on evolution the two words development and evolution are used as synonymous terms.

We may then resolve the question into this concrete form: In the case of any particular organic cycles of phenomena, is it more simple and fundamental for the organism to reproduce its kind or to produce itself, *i. e.*, its mature self from the germ? We can logically find but one reply to this question. Production must precede reproduction. But what does this answer imply? It implies that the processes of development of the individual from the germ to the adult do, in their intrinsic nature, precede the phenomena of reproduction. It further implies that the phenomena of evolutional variation are supplementary to, and then a further pushing on of, the phenomena of individual development. The assumption, which is generally accepted, appears to be that this mode of variation is a modification of ordinary reproduction, either produced

by some spontaneous action of the organism, or the result of the influence of changing environment upon the organism. For, in speaking of variation, it is customary to say that the variation becomes fixed by selection, or becomes transmitted, as if the disturbance in the order of the phenomena, which at first entered as an accident, became, by some means, an added part of the normal cycle of development of succeeding generations.

We hear such expressions as that it is impossible that a variation can be transmitted till it affects the germ-plasm, or till the variation becomes a variation of the reproducing, as distinct from the somatic, part of the individual. This conveys the impression that that which varies is the reproductive cycle of phenomena, whereas the truth is more accurately expressed by saying that the reproduction cycle is augmented. The augmentation in the case consisting of an extension of the process of growth of the individual beyond the point reached by the ancestor, the process is, first, purely of the nature of the building up of tissue and structure, and is not reproduction, but simple production, the process of production going beyond the extent or limit reached by the ancestors.

The confusion arises from not distinguishing the phenomena by which the structure of the individual is perfected from other phenomena by which a new individual becomes separated from the old and begins, carries on and repeats the previous cycle of phenomena. That phenomenon which is the first step of every evolutional process, as well as every step by which evolutional progress is accomplished, is fundamentally a growth phenomenon, quite of the same nature as the growth taking place throughout the life of each individual. It differs from these normal growth phenomena only by exceeding in some particular, or deviating in some manner, from the cycle of growth phenomena of the ancestors. Evolution does not call for any augmentation of the phenomena of reproduction.

If we separate the processes of (1) the growth or development of one individual from (2) the reproduction of a separate second individual we discover that the development of the first individual must necessarily have been carried to a certain point of completion before the reproduction of a germ takes place, since it is the more or less mature individual which reproduces the germ. When, further, we compare with these two processes the further phenomenon of variation which results in evolution we find that the variation does not belong to the reproduction, but to the development of the individual. Variation is a transcending the course of development of its ancestor by the offspring ; reproduction of the variation is not variation, but a repetition of a previous course of development. It is simply a continuation of those processes which have been going on in the individual and are regarded by the observer as perfectly normal up to the point of reproducing the features of the ancestor, but are looked upon as abnormal so soon as they transcend their limits.

Inquiry into what we mean by normal and abnormal will reveal the commonly received doctrine in the case. By normal we mean according to the steps of growth of the ancestor ; that is to say, the assumption is made that it is natural or normal for reproduction to proceed in some path already traversed. Now, in fact, this is not strictly true; first, we know that species are constantly showing departure or 'abnormal' growth (using abnormal in the above sense), and the deviation is called variation; and secondly, we have reason to believe that organisms never proceed in exact imitation of anything else, that every part of every organism is in some infini-

tesimal sense different from any other. This is really a distinctive feature of organisms as contrasted with bodies of inorganic matter.

This distinction between normal and abnormal reproduction, as if variation were but a slight modification of the so-called law of reproduction, has also led us into confusion. Reproduction is but the production again of what has been produced in a previous cycle; and a case of variation in the offspring, however slight, is not a case of reproduction, but of the production in the offspring of some new character, and the great thing to account for is the fact of the production of such new characters in organisms. But the process by which the individual acquired some new character is not different in nature from the process by which it acquired the old characters already expressed by its ancestors. If we can account for growth in the first place we are on the immediate track of accounting for the continuance of growth. To say that the growth of the individual in a particular direction and to a particular degree is due to the influence of the ancestor upon the offspring is offering a cause for reproduction, but not for variation; for, however variable the original stock might be, generation would result in increasing the degree of uniformity of the ancestry of each individual. As one can easily discover by computing the total number of direct ancestors of any individual with two parents, and supposing them to belong to distinct lines in each generation, it would take but twenty-one generations back to find one's lineage spreading over a million separate individuals of the twenty-first generation. If the ancestors controlled the growth of the offspring it is thus evident that, however different might be the individuals at any particular period in the course of a hundred generations, given free access to crossing, each offspring would unite strains of influence from every possible line of ancestry which had been accessible.

If variation were the result of difference in the external conditions, or what we call environment, the question arises why should not the same variability be expressed in the phenomena of crystallization; in the phenomena of chemical combination of elements; in the phenomena of light or heat, and in all the physico-chemical phenomena of matter, where like conditions produce like effects? If we have a uniform common force at work, the varying expressions of which are due to diverse conditions of environment, why should the result be so different from any other uniform common force operating under like diverse conditions of environment? The question brings its self-evident answer; the variations cannot be explained as the reflex of a discrete and varying environment upon a uniform common kind of matter. The idea that the cycles of development of the offspring should repeat the cycles of development of the ancestors is based on the prior assumption that the organism does not normally vary; that it acts as if it were an inorganic body, subject to the law of inertia and conservation of force. With this idea, it is easy to imagine that the cycle, once started, should not stop, except by reason of some resistance or impediment.

But we ask how can the cycle begin? How can it be started? and here we come to the fundamental point under consideration. Starting is itself variation—a departure from remaining inactive; and a cycle is uniformity, not variation. If the simplest act in the world takes place, it is a diversion from the condition of things before it took place; and if it stop and is simply repeated periodically, there is a cessation of the action of the initial starting force, and we have but the continuation of reflex action of the original impulse in the midst of resisting media. Hence, to begin

a cycle of phenomena, of whatever kind, requires the initiation of the original variation in which it began. The same is true of any variation thereafter, if we are to apply the reasoning which is valid regarding physico-chemical laws of matter to the phenomena of organisms. If the variation be primitive, and normal, to use the word in the sense proposed, it is evident that what we call reproduction is but a pulsation of the phenomena of life itself, recurring in the precise cycles which express the equilibrium between a definite quantitative force in the individual organism itself and the definite quantitative forces of the total environment in which it carries on its life functions.

The preservation and perpetuation of derived characteristics in a race seem at first sight to be easily accounted for by the process which Darwin has called natural selection. Variation, in this hypothesis, is supposed to occur 'naturally,' by accident, or, as Darwin says, 'spontaneously.' But a close examination of what such a proposition would mean in concrete facts reveals serious difficulties. The apparent simplicity arises from the assumption that the law of hereditary transmission of ancestral characters is a primary law of organism, which is violated in every case of variation. On such an assumption we have only to conceive of the removal of whatever may have occasioned the accidental or spontaneous disturbance in order to permit the continuing on of the normal working of heredity. But when we follow the hypothesis back to its beginning, it provides no means for rising from the original level of simplicity. Each variation must, according to the theory, be a violation of the normal action of the organism; hence if the organism were adjusted when the variation took place the variation puts it out of adjustment, and we have no place for the action of natural selection. If, on the other hand, the variation is advantageous to the economy of the organism, then we must assume either that the organism was not in perfect adjustment when it varied; and then again the adjustment is accounted for without the action of natural selection, or else the lack of adjustment came from change of conditions. In this case the conditions of environment, not natural selection, account for the adjustment. And there seems to be a still greater difficulty, viz., the extreme length of time necessary to bring about the changes that have taken place by the process. Recently Professor Poulton called attention to the necessity of this great time period (longer than the physicists or geologists are generally ready to allow to have been possible), in order to account for the results we find recorded in the fossil-bearing rocks, requiring at least 400 millions of years for the work of evolution.* But Mr. Poulton does not exaggerate the matter.

Let us examine this time factor and see if we can imagine it to have been long enough. In the first place, if hereditary repetition be the normal law of organism, then Professor Poulton has made a fair estimate of the ages it would take according to the present rate of evolution. But he has not taken into account all the necessities of the theory, two of which must be these: First, if the exact hereditary transmission and repetition be the fundamental law of organisms, not only must the progress produced by any mode of variation have been exceedingly slow—at first at a rate decreasing geometrically in proportion to the greater simplicity of the organism; but second, the theory requires that if natural selection consists in making variations permanent, the general progress must take place by means of a process which in every particular case consists in stopping the very phenomena by which the progress is at-

*SCIENCE, Vol. 54, p, 504.

tained. It is, moreover, this becoming permanent, by hereditary acquirement of the variation, which constitutes the evolutional progress of the series. And the difficulty we meet with is that we are assuming that natural selection must actually check, or even stop that variational activity by means of which any change whatever is attained, according to the theory of a primitive law of heredity. But if we were to grant that progress could be attained in this way, and allowing the slow rate of the actual process by which a variety becomes permanent enough to be called a strain or set habit of a race, and granting Professor Poulton's demand of the necessary four hundreds of millions of years for the process at the present rate—granting all this to be possible, we have still to reckon with a still more important process, the raising of the functional importance of the new varietal modification to reach a rank of specific, generic, family, ordinal, and, before we are through with it, class, branch and subkingdom value in the individual economy. The time required for this would be practically infinite. Because, with each step in advance in taxonomic rank and importance, the rigidity of transmission must be supposed to become greater, and thus the degree of possible variability diminishingly less. This would result, even if we were to grant that the change in taxonomic rank of the character be a fact.

But the evidences of paleontology go to disprove the very matter of fact. As has been already pointed out in another place, the degree of differentiation and the classification of invertebrates of the first great era in which we have definite records of organic life are so closely in conformity with that which we know of the invertebrates of the same classes now living that all the distinctions necessary to be considered in an ordinary course of lectures to a class of students in invertebrate zoology, to-day, would apply, so far as the facts are recorded, to the organisms which lived in the earliest period of which we have definite record of any living organism on the face of the earth ('Geological Biology,' p. 212). This evidence means that the same kind of characters, which are varietal and specific characters in living organisms to-day, were varietal and specific characters in the representatives of the same classes back in the Cambrian time; that the same kind of characters which are now generic in rank were then generic characters. And, so, in the case of family, ordinal and class characters we discover no trace of evidence that characters bearing a particular rank in the organic economy now, among living beings, did not always bear the same relative position among the characters of the bodies of their ancestors.

Attention was called to these facts several years ago and their validity does not appear to have been questioned. We observe, further, that in Cambrian time the differentiations of animals of branch value had already taken place, with the exception of vertebrates; and vertebrates appeared in the Ordovicean. And in the case of the vertebrates of the Ordovicean (and only a single locality for them is as yet known) their representatives are distributed by experts into three of the five known (*i. e.*, in fossil condition) sub-classes of Fishes. Fishes, it must be observed, include the type of vertebrates which are adjusted alone to an aqueous environment, and, therefore, we may conclude that, so far as the vertebrates of the environment of which we have any record for that era are concerned, they had reached over one-half the differentiation of sub-class rank ever attained by them.

Lest there should appear to be a misrepresentation of the opinions against which these arguments are directed, quotation from Darwin's 'Origin of Species' on

this point may appropriately be inserted here.

Darwin wrote:

"Hence I look at individual differences, though of small interest to the systematist, as of high importance for us, as being the first step toward such slight varieties as are barely thought worth recording in works on natural history. And I look at varieties which are in any degree more distinct and permanent as steps leading to more strongly marked and more permanent varieties; and at these latter as leading to sub-species and to species. * * * I attribute the passage of a variety from the state in which it differs very slightly from its parent to one in which it differs more, to the action of natural selection in accumulating difference of structure in certain definite directions. Hence I believe a well-marked variety may be justly called an incipient species (p. 53).

"Therefore, during the modification of the descendants of any one species, and during the incessant struggle of all species to increase in numbers, the more diversified their descendants become, the better will be their chance of succeeding in the battle of life. Thus the small differences distinguishing the varieties of the same species will steadily tend to increase till they come to equal the greater difference between species of the same genus, or even of distinct genera (p. 117).

"Natural selection acts, as we have seen, exclusively by the preservation and accumulation of variations, which are beneficial under the organic and inorganic conditions of life to which each creature is at each successive period exposed" (p. 117).

As if to make the inadequacy of this conception more apparent, we have but to look back across the geological ages, or, accepting the law of recapitulation, to trace the embryonic development of a single higher animal, in order to discover that the earlier differentiations were of actually higher rank, and that as time has progressed the new forms of organisms have been restricted to modifications of less and less importance. The earlier in time we go the more fundamental were the variations which took place, and it is in later geological times that there has come to be more and more rigid adherence to the law of heredity.

The proposed theory of original variability is not only consistent with such a series of events, but they would be the natural expression of such a force in operation. Variability should be most active and most vigorous before the laws of heredity had restricted its action. We must not, however, confuse activity of the operation of this law with multiplicity or complexity of activities in a common body. Complexity of structure is a matter of development and adjustment of the body itself, and much collateral evolution would be necessary before it would be possible for great complexity in a single body to be consistent with the limits of its vital functions. That the changes and adjustments would be great and rapid in proportion to those that followed when the adjustments had become close and involved is, however, evident. Hence it would be consistent to expect rapid evolution at first, gradually decreasing in rate with advance of time, as paleontology teaches us to believe was the actual fact of the case.

The difficulty in the commonly accepted view, it seems to the author, arises from mental confusion rather than neglect of the real phenomenon in the case. The mental juggling takes place when we speak of varieties or variation becoming more permanent, or when we speak of the preservation and accumulation of variations.

Variation as an act means becoming different, but variation as a thing means

something which does not vary. Permanent as an adjective means lasting and enduring, and thus it is contrasted with the adjective sense of the term variable. Thus when Darwin speaks of natural selection as acting by the 'preservation and accumulation of variations,' there is nothing variable in that which is conceived of as being preserved and accumulated. It is a character or morphological structure which is preserved and accumulated in the offspring only when it is the same character which appeared in the parent form. It is the fact of the reappearance of the same character in the offspring which is meant by its preservation. It will be seen, thus, that the origin, or arrival into the organic structure, of the particular concrete variation which, in any particular case, is transmitted and preserved must necessarily have taken place before natural selection acts in that particular case. Therefore, the variation, as an act, or the actual becoming different, is of a two-fold nature: (1) It consists, first, of the growth of some part of the structure of the organism in some way and degree differently from the growth of the same part in the ancestor; and (2), secondly, there is the reproduction of that difference in the offspring in accordance with the growth as it took place in the parent or immediate ancestor. Here, in the act, we see again a confusion of two acts, one of which is permanent and the other variable. The first act is a diversion or contradiction of the law of heredity, the second act is in conformity with it. The true variation, as an act, is thus a real departure, or diversion, from the phenomena of hereditary repetition. It is this which I understand Darwin assumed to have been spontaneous or constantly occurring, and it is the operation of natural selection, chiefly, and of other agencies working upon the living organisms, which, according to Darwin, results in the increasing diversity of the individuals. We are thus led by an analysis of Darwin's own theory to find that the real variations occur prior to any of those operations of the organism, or of the environment, commonly supposed to have caused them.

Darwin's theory, nevertheless, is readily adjusted to the conception of the fundamental nature of organic variation here proposed. It requires but an expansion of the idea of mutability of species so as to include mutability of the individual and of organic matter itself. Natural selection is constantly producing heredity, not variation. But natural selection is not the only cause; environment in general, and we might extend the idea of environment and say that experience is constantly resulting in the hereditary transmission of qualities or characteristics from parent to offspring.

The definite laws of heredity for any particular organism at any particular point in its history are but the recapitulation of the experience of its ancestors in overcoming, conquering and using for their enrichment the impediments and constantly acting hindrances to their living and existence. Varying is the first, as it is the last, performance of the living being. Invariability is the law of the inorganic world, but is the sign of death among organisms. This thought was aptly expressed by the late James D. Dana, in the last revision of his 'Manual of Geology.' Speaking of variation among organisms he wrote: "It is perceived that the law of nature here exemplified is not 'like produces like,' but like *with an increment* or some addition to the variation. Consequently, the law of nature, as regards the kingdoms of life is not permanence, but change, evolution" (p. 1033).

A rational and consistent conception of organic evolution arises from this theory of the fundamental nature of variation in organisms. Evolution is, to this theory, only the extension of the phenomena of

growth, or development of the individual beyond the point reached by its ancestors. Natural selection operates in the manner set forth in the current hypothesis, only the result is confined to holding in check and regulating the cycles of individual development, not to producing them. Environment affects the organism, both directly and indirectly, as Lamarck and the Neo-Lamarckians claim, but the effect is in the way of checking and then controlling variation.

The organism is in all respects dependent for its resources upon environment. Living is a constant process of occupying, using and discarding matter, and therefore any structure or function developed by the organic body is either profitable to the continuance of the living individual, or it is not profitable. Any modification of structure has a definite economic value to the individual; if its benefit does not equal its cost in energy its production is an unprofitable venture and is either not repeated or the individual is crippled and finally lost by the operation of natural selection. If the organism, for any cause, acquires surplus energy it is expressed in variation, and if the variation is to the advantage of the individual, *i. e.*, if the resources of new energy resulting from its presence exceed the expenditure for its construction and maintenance, the result is beneficial and the new structure is retained and a step in advance is made.

Thus the condition of environment, from the old point of view, seems to cause the organism to vary; in the new view, the organism adjusts and keeps adjusted to its environment by the law of internal economy, not by the external struggle for life.

It is not necessary, here, to suppose that there must be a specific conscious adjuster residing in each organism. We do not find it necessary to imagine a specific *erdgeist* in order to cause the earth to follow the intricate curve of its revolution among the other planets and about the sun. Nor is it necessary to assume, as Professor Bailey puts it, that "definite variation is an inherent or necessary quality of organic matter,"* but given this general law of variation as an intrinsic property or mode of operation of every particle of living matter, and the phenomena of life will result—in the lowest stage as metabolic phenomena, then in the second stage as individual development and, also, in the third stage as evolution, by simply continuing their activities. And the same power which can constitute variation among the phenomena of matter, otherwise controlled by the inflexible physical laws of inertia and conservation of force, can, doubtless, institute in living matter that still higher function, consciousness, with all the wonderful phenomena which are associated with it.

The significance of this theory is considerable, both scientifically and philosophically. From a scientific point of view, variation or variability is recognized as the very essence of the vital phenomena, as gravitation is recognized as an essential characteristic of matter. Life is as remarkable (but perhaps no more so) as that sudden demonstration of expansion which inelastic water or rigid ice exhibits when raised to 212 degrees of Fahrenheit. We might study ice and water for eternity under temperatures below the boiling point and never discover the properties of steam So, whether the vital phenomena are latent in matter or not is a matter of speculation. Whenever vital phenomena appeared they appeared in phenomena exhibited by matter. Whenever inorganic matter becomes vitalized, however that result may be accomplished, variation takes place and distinguishes it from matter in every other condition.

*'The Survival of the Unlike,' p. 22.

If anything be evolved by evolution it is evident that, whatever its nature may be, it must cease to be evolved if it would maintain its integrity. For inertia of matter and conservation of force apply to bodies which no longer are undergoing evolution. Variation, as a process of becoming different, is a characteristic of living bodies, and, though it is not doubted that in the phenomena of variation it is ordinary chemical and physical matter which exhibits the peculiar vital phenomena, we have no reason to suppose that the operations of physics and chemistry are thus variable.

H. S. WILLIAMS.
YALE UNIVERSITY.

MICROSCOPICAL EXAMINATION OF WATER, WITH A DESCRIPTION OF A SIMPLE FORM OF APPARATUS.

THE microscopical examination of water is becoming every year a matter of greater interest, and the study of the minute aquatic plants and animals is more and more attracting the attention of scientists. These organisms are interesting for several reasons and, besides recognizing their importance in the domain of pure science, we are beginning to appreciate the great part that they play in nature and their effect, direct and indirect, upon the human being. Their presence in surface waters is often the cause of much harm when the water is used for purposes of domestic supply; scores of instances may be mentioned where they have rendered the water entirely unfit for use. On the other hand, their presence in ponds and streams is of importance to the fish-culturist because they form the fundamental source of the food supply of fishes; this is probably true both of salt and fresh water.

Because of this connection between the number of microscopical organisms in a cubic centimeter of water and the price of fish in our markets, the study of the 'plankton,' *i. e.*, the floating micro-organisms, is being emphasized on both sides of the Atlantic. Observers are beginning to trace the connection between the presence of microscopical organisms and the abundance of fish in our lakes, and valuable comparisons have been made between the stomach and intestinal contents of fishes and the organisms found in the water where the catches were made. This work is of very great importance and should be vigorously pursued by our fish commissions. To be of the greatest value it should extend well over the country and include lakes and ponds sufficiently different in character to enable one to determine the laws governing the nature and distribution of the plankton in various climates and under various conditions. The study ought not to be carried on spasmodically, as, for instance, during the short vacation of some college professor who generously gives his time and talents to the cause, but should be undertaken seriously and continued throughout the whole year. Only in this way can we obtain the data necessary for a complete understanding of the subject.

Since water works managers are equally interested in the microscopical organisms found in surface waters, and up to the present time have been responsible for most of the work done upon the subject, it might be possible for fish commissions, boards of health, water-works superintendents, and others interested, to work together according to a definite concerted plan, sending their results to some central commission or committee for comparison and study. Such an extended biological study taken in connection with meteorological records and observations upon the temperature, transparency, etc., of the water would be of very great value. And it would seem that we have little excuse for neglecting to cultivate this fruitful field of research. Vast num-

bers of microscopical examinations are now being made [during the past eight years more than 40,000 have been made in Massachusetts alone], and the rapid growth of the new science of sanitary biology is developing numbers of well-trained observers wide awake to the value of these problems and well able to undertake the work. What is needed is cooperation.

Various methods have been employed from time to time for determining the character and amount of microscopic life in water. Those interested in the subject from the piscatorial standpoint have usually employed some sort of net for straining the organisms from the water and concentrating them for the microscope. One of the best devices of this kind is that devised by Professor Reighard and used with good results for studying the plankton in Lake Michigan. It consists of a conical net of fine bolting cloth, at the small end of which there is a 'bucket,' made by covering a metal framework with some of the same bolting cloth. The apparatus is hauled through the water, filtering a column of water whose cross section is the same as the circular mouth of the net and whose length is equal to the distance through which the net is hauled. The organisms are caught by the fine bolting cloth and are ultimately washed into the bucket. The collected material is then removed by an ingenious arrangement, measured and sent to the laboratory for microscopical examination. By this method one is enabled to get a good idea of the total amount of suspended matter in the water, but it can hardly be called an accurate method of obtaining the number of living organisms present, as the net sweeps in amorphous matter as well as organisms and some of the smaller forms undoubtedly escape through the bolting cloth. Moreover, the amount of water actually filtered cannot be told with a great degree of accuracy. Nevertheless, the method is one of value, particularly for securing the larger and rarer forms of rotifers, crustacea, etc.

Sanitarians who have studied the microscopical organisms in water supplies have usually employed very different methods from the above, partly because they have been interested more especially in the smaller forms, but chiefly because their operations have been confined to the small quantities of water sent to the laboratories for analysis. During the last decade the old methods of sediment examination have given way to the filtration methods. The Sedgwick-Rafter method, which is most used at the present time in laboratories of water analysis, is carried on as follows:

A portion of the water to be examined is measured out in a graduate and filtered through a thin layer of quartz sand placed at the bottom of a glass funnel upon a perforated rubber stopper, the hole in which is capped with a disc of bolting cloth. When the water has filtered, the organisms will be found upon the sand, while the filtered water will be free from them. The rubber stopper is then removed and the sand washed into a test tube, with a measured quantity of distilled water delivered from a pipette. Usually 250 or 500 c. c. of the sample are filtered and the sand washed with 5 c. c. The test tube is then thoroughly shaken and the water decanted into a second tube; the organisms being lighter than the sand, will pass off with the water, leaving the sand clean upon the walls of the first tube. In this way the organisms are concentrated 50 or 100 times. One c. c. of this concentrated fluid is then transferred to a counting cell, which just holds it and which has a superficial area of 1,000 sq. mm. After putting a thin glass coverslip over this cell it is transferred to the stage of the microscope for examination. The eye-piece of the microscope is fitted with a micrometer in the shape of a ruled

square of such a size as to cover one sq. mm. on the stage, *i. e.*, one thousandth of the entire area of the cell. The organisms observed within the limits of the ruled square are then counted and the cell moved until another portion comes into view, when another count is made. Thus 10 or 20 squares are counted and the number of organisms present in the sample calculated.

This process has many things to be said in its favor, and it is undoubtedly the best all-around method for the study of the plankton. The apparatus required is simple, inexpensive and not liable to get out of order. The process is neither long nor difficult, and if care and cleanliness are observed in the manipulation very accurate results may be obtained. Ordinarily the quantity of water operated upon is small, but there is no reason why large filters may not be used. The writer has frequently used a funnel having a neck one inch in diameter, filtering from 1,000 to 10,000 c.c. This, when used with an aspirator to hasten the filtration, has given excellent satisfaction. The chief objection to the Sedgwick-Rafter method is that delicate organisms are liable to be crushed upon the sand, and this danger is naturally somewhat greater when this aspirator is used. It is probably no greater, however, than in Reighard's net.

Recently a new apparatus has been devised for the study of the microscopical organisms, known as the planktonikrit. This is a modification of the centrifugal machine and depends upon the fact that the specific gravity of the organisms is different from that of water. It has the advantage of avoiding, to a certain degree, the crushing of the delicate infusoria, but it is somewhat inaccurate in the case of some of the lighter organisms; furthermore, it operates upon very small quantities of water.

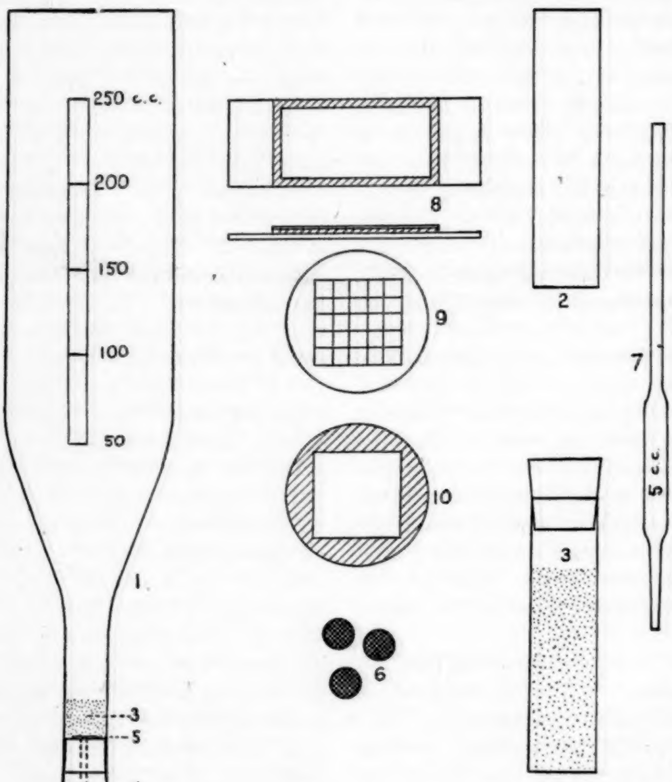
In a complete study of the microscopical organisms, such as might be undertaken on our great lakes, for example, it would be advisable to use all three methods, adopting the Sedgwick-Rafter method for general quantitative work, but using the net and centrifugal apparatus for determining the rare and delicate organisms.

As there are many lovers of the microscope who are interested in studying aquatic life, and as there are many others connected with water-works to whom the study of algæ and infusoria would be of much value, the writer has tried to reduce the Sedgwick-Rafter method to its simplest possible elements in order that it may be more generally used. Furthermore, it is often necessary for the sanitary biologist to be provided with a portable outfit for work in the field. There are many fragile organisms which will not bear transportation to the laboratory. Uroglena, for example, a very important and troublesome organism found in water supplies, goes to pieces completely when kept for a short time in a stoppered bottle. It is, therefore, necessary to make the examination of water immediately after the collection of the sample.

The chief modification of the method for field work consists in the use of a cylindrical glass funnel [Fig. 1] similar to the one designed by Mr. D. D. Jackson for the Massachusetts State Board of Health, but differing from it in having a capacity of 250 instead of 500 c.c., and in having graduations marked upon the sides. This funnel may be conveniently carried and its graduation renders the use of a second measuring glass unnecessary. When in use it may be supported on a wire frame, which any ingenious person can make In place of the test-tube it has been found convenient to use tube vials [Fig. 2] having square ends. These require no racks and are not easily tipped over. The pipette for washing the sand might be dispensed with if one of the tube vials was graduated, but as much depends upon ac-

curacy in concentrating the sample it is best to use a short pipette [Fig. 7]. The sand [Fig. 3] used in the filter should be perfectly clean and of such size that its grains will pass through a sieve having 60 meshes to the inch, but not through one having 100 meshes. Crushed quartz makes the best filtering material and should be for holding the concentrated fluid may be made by cementing a brass rim to an ordinary glass slip. It should be 50 mm. long, 20 wide and 1 mm. deep, thus holding just 1 c.c. and having a superficial area of 1,000 sq. mm.

A very simple microscope will answer for this work. A large stand is too valuable



used when obtainable. The discs of bolting cloth [Fig. 6] may be easily cut out with a wad cutter. The filtered water may be used for concentrating the organisms, or it is possible to employ preservative fluids in case the microscopical examination must be deferred or it is desired to keep the specimens. The cell [Fig. 8] and too heavy for the rough usage in the field, and a cheap, light stand with a $\frac{1}{2}''$ or $\frac{2}{3}''$ objective and a No. 3 ocular will answer equally well. The ocular must be provided with a micrometer, so that the observer may count the number of organisms in one cu. mm. of the cell. A disc of glass ruled as in Fig. 9 is the best form of micrometer,

but a piece of thin metal with a square cut out, as shown in Fig. 10, may be substituted. In either case the square must be of such a size that it covers one sq. mm. on the stage with a given combination of objective and ocular, and a certain tube length to be found by comparison with a stage micrometer. It is an advantage to have at hand higher powers for a more thorough study of the organisms met with, but for ordinary work the powers suggested are sufficient.

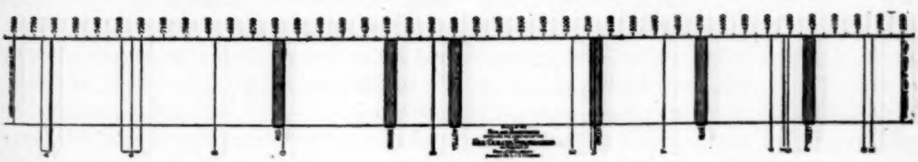
All this apparatus, together with bottles for collection and note book for records may be carried in a grip sack, and this will be found generally the most convenient way. It is possible, however, to make a neat box, with compartments for holding the microscope, funnels, tube vials, etc., and I respectfully submit this to manufacturers of microscopical supplies.

GEORGE C. WHIPPLE.

NEWTON CENTRE, MASS.

SPECTRUM COLOR STANDARDS.

THE extensive adoption of the *color standards* proposed by me and put into practical form for educational purposes by Mr. Milton Bradley, of Springfield, Mass., leads me to offer the readers of SCIENCE an opportunity to examine a chart of the solar spectrum after Rowland with the standard colors located upon it. The importance of making the spectrum the basis of all our work in color is recognized by all, and I have received many appreciative communications from eminent men in scientific and educational circles, both in this country and England, expressing approbation of the effort to bring our now greatly confused color nomenclature within some accurate and practical system. The idea of teaching color by a system thus definitely defined has also proved to be very practical, not only in elementary instruction, but in the more exacting art work. This rapidly increasing public interest in the subject makes it seem likely that the accompaning chart will be of interest.



A few observations on the practical application of these standards will illustrate the value of the scheme. The area representing each particular standard in the chart is narrow enough to allow of no perceptible difference in the hue of the two sides of the area when viewed through the spectroscope and is still wide enough to give a clear working field. Moreover, the areas selected coincide with the views of a large number of persons experienced in the discrimination of color and well prepared, therefore, to judge what would be of practical value as color standards and applicable to the various needs of the arts as well as to science and to educational purposes. And still again, though there was no direct reference to the theories of color vision, these standards having been first proposed nearly fifteen years since, the standards prove to have been happily selected as regards the more recent theories of color vision. Another very important consideration is the fact that the quality of any color which I have yet seen can be obtained by the union of two of these standards. Of course, the intensity of one of the two compared colors will generally have to be modified to obtain a perfect match, but I have never yet been unable to do this. In a

series of comparisons now before me, including many of the brilliant colors of silk ribbons recently so fashionable, I have had to reduce the intensity of the color to be reproduced, in only two out of sixty cases. This comparison was made by means of Maxwell discs made from the Bradley educational papers. For all practical purposes some form of paper or card board disc seems more convenient and proves quite satisfactory, as the above statement shows While it is possible to get somewhat more of brilliancy in silk than in pigment-covered papers, I have not found the increased brilliancy of sufficient value to compensate for the difficulty of making silk-covered discs that can be conveniently operated.

For educational purposes it seems to me of first importance that the child (and many adults are children in this regard) should start out with some clear and correct idea of a few pure spectrum colors. It does not matter essentially just how many of these colors he is taught, only that he gets a definite conception of those of which he learns. The reason for taking six for this series of standards was that I found that it was very difficult to get any practical scheme of color teaching based on a less number of standards. The mixing of two standards by means of the color wheel always reduces the strength of the resulting color. For example, the mixing of red and yellow on the wheel will produce orange, but it will not be as strong an orange as that of the standard paper. It will be a broken orange. Using the nomenclature adopted in my former articles (SCIENCE, February 26, '92, and June 9, '93) we have the result represented as follows: R 82 Y 18 gives the same effect as O 55 W 2 N 43. This can be best tested by using two sizes of discs. The larger should be compounded of red and yellow in the proportions just given and the smaller of orange, white and black as indicated. In this way the result is very satisfactory in fairly strong and pure diffused sunlight. The same principle will apply to all the colors of the spectrum. But the extent to which the effect of a broken color is produced will be increased, as the colors differ more widely in luminosity, or are more widely separated in the solar spectrum. Hence a smaller number of standards makes the practical use of the standards more difficult in the pupil's study of the colors by the use of the Maxwell discs. If we take a larger number of colors than six it becomes more difficult to designate them clearly in the solar spectrum, and the system becomes more complex. As there is quite general agreement that indigo shall not be used as a name for a spectrum color, the most natural and far the most convenient terms are those used in the scheme already referred to. When the pupil becomes familiar with these standard colors he is prepared to make very rapid progress in color study. The combination of these standards will give a ready appreciation of the nature of the purer color in objects about him and prepare the way for the analysis of colors, first those of a pure tone and afterwards of broken colors. The next stage would be the teaching of color harmonies. This most difficult part of the work becomes relatively easy by means of educational papers constructed on so simple a plan as this. It is surprising to see what results are obtained in the schools where this system has been followed.

It seems not inappropriate in this connection to call attention to an attempt to apply the principles upon which I have worked with a somewhat different set of standards from those I have used under the article 'spectrum' in the Standard Dictionary. In the first place there is no pure red used in the system, that called red being an orange red, as represented by the pigment vermilion. Nor is there any violet

used in the scheme, except by an unintended blunder, by which the wave-length of ultramarine is given as .4250, which would bring the standard blue designated into the clear violet of the spectrum and not distinguishable from the violet of my standards. The wave-length of ultramarine is not far from .4500. With no pure red or violet in a scheme of standard colors it is quite impossible to obtain or to represent accurately a very large line of important hues. The author of the article has been obliged, because of this defect in the standards he has selected, in many of the analyses of popular colors given in the table accompanying the article, to use three or four standards to represent the color he has analyzed where two pure spectrum colors are all that are needed. It is to be regretted that a publication like this should have added anything to the confusion which we are seeking to remedy. And this is still more to be regretted because the publishers of the Dictionary asked for and received from me the measurements and explanations of the plan upon which I had been working with the professed purpose of furthering the extension of the plan. This will not be as serious a matter, however, as it otherwise would have been, since the rapid introduction of the Bradley papers into the public schools gives an opportunity for the correct teaching of color to an enormous number of children.

J. H. PILLSBURY.

STONEHAM, MASS., June, '97.

PLEISTOCENE FOSSILS FROM BAFFINLAND AND GREENLAND.

THE existence of Pleistocene deposits in Labrador,* in several of the islands of the Arctic archipelago,† and in northern Greenland,‡ has been made known by several observers, but, so far as the writer is aware, none have previously been reported from Baffinland. The discovery of Pleistocene shells during the past summer on the south coast of Baffinland is, therefore, of interest as showing that at least a part of this island, which is the largest of the Arctic archipelago, was also affected by the subsidence which lowered the lands to the north and the south of it beneath the sea during the Pleistocene.

* Mem. Bost. Soc. Nat. Hist., Vol. 1, pp. 229-230.

† Jour. Geol. Soc., Vol. 9, p. 317.

‡ Jour. Geol. Soc., Vol. 3, p. 100.

Short trips were made into the interior by members of the Cornell University party from two points on the coast north of Hudson Strait. The first of these excursions was from a point opposite Big Island, in about long. 70 W., and the second some ten or twelve miles east of the first. At both localities Pleistocene shells were found in small lake basins a short distance from the coast at elevations from 100 to 200 feet above sea level. At two localities the shells were found in abundance, occurring in a fine blue clay mixed with some sand. These fossiliferous deposits do not occur in all of the basins and valleys, which are very numerous in this region. In most of them they appear to be absent.

The following is a list of the species arranged in the order of their abundance, which were obtained from a deposit of blue clay about 150 feet above sea level:* *Saxicava arctica*, *Mya truncata*, *Rhynchonella psittacea*, *Astarte compressa ? Balanus crenatus*, *Pecten icelandicus*, *Lepeta cæca*.

In the interior of Big Island, Mr. T. L. Watson found Pleistocene shells at an elevation of 270 feet. The species which he obtained are *Mya truncata* and *Macoma sabulosa*.

The Greenland shells are especially interesting because of the peculiar manner of

* The writer is indebted to Professor H. S. Williams for the use of specimens for comparison, and to Miss Katharine J. Bush for assistance in the determination of species.

their occurrence. All of them were obtained from the moraine fringing the margin of the ice cap along the base of the Nugsuak peninsula. The peninsula is a narrow mountainous tongue of land extending a little south of west from the mainland. Glaciers extend down to the fjiords from the ice cap on either side of the Nugsuak peninsula, while along its eastern base the ice cap terminates generally in a gently sloping margin, but sometimes in vertical cliffs forty to eighty feet high, which face small ponded lakes. From the Cornell glacier the margin of the ice cap rises gradually for a distance of four or five miles until it reaches an elevation of about six hundred feet, and then gradually descends again toward the glacier entering the fjord to the north. The morainal material, which occurs everywhere except at the lakes, just at the edge of the ice, was found to contain shells or fragments of shells up to the highest elevation which it attains, about 590 feet, at a distance of about four or five miles from the coast. At some of the small lakes the ice cliffs show distinctly stratified ice which carries an abundance of débris in the lower layers. Shell fragments were observed in the face of one of these cliffs at an elevation of 390 feet above sea level. Broken shells were also observed in the morainal material which has been dumped on the surface of the ice at a few points, owing to the sharp dip of the ice strata which brings the lower débris-carrying layers to the surface before they reach the outer margin of the melting ice.

The following species have been identified in the material collected from the moraine: *Saxicava arctica*, *Mya truncata*, *Macoma subulosa*, *Balanus crenatus*, *Yoldia* (*Portlandica*) *arctica*, *Cardium ciliatum*.

Many of the shells from the moraine near the fjord, which have evidently been carried but a short distance, have been handled in a surprisingly delicate manner by the ice. The *Macomias*, which are so fragile as scarcely to allow removal from the soft clay without crushing, have in many cases escaped from the ice without the slightest injury. The only specimen of *Yoldia* found still retained the epidermis, and the valves remained attached. In following the moraine back from the fjord toward the higher land which separates the Cornell and the Wyckoff glacial basins, the shells in the moraine become more and more fragmentary and broken as the irregularity of the land topography underlying the ice, and the distance which they have been transported, increases.

The occurrence of these shells in the moraine and in the ice makes it clear that they have been picked up by the ice from an old sea bottom which is now occupied by the ice cap. From the position of the shells and the direction of the ice flow which has transported them, the sea must have extended up the fjord when the shells were living at least four or five miles beyond the present face of the glacier. There seems to be only two possible interpretations of the relation of these shells to the ice. They either belong to a time previous to the beginning of extensive glaciation when the sea extended up the fjords as far as their slopes would permit, or they represent a retreat of the ice which allowed the sea to extend some miles up the fjord beyond its present limit, followed by a re-advance of the ice. If the former supposition be correct, the removal of shells must have been in progress from the beginning of glaciation in the region to the present time. It would seem that so long a period of excavation by the ice would have exhausted the material from so limited a supply. According to this theory, morainal material formed at an earlier period, when the ice extended beyond its present position, ought to contain shells as well as that now forming. I care-

fully examined such material for shells, but found no traces of them. These shells do not then belong to a preglacial fauna. The true interpretation of their history seems to be that there has been a retreat of the ice front some miles beyond its present position and a re-advance.

The remaining problem is, to what was the retreat and re-advance due, and when did it occur? The retreat was probably caused by the general subsidence in the north which is indicated by elevated deposits of recent shells in Baffinland, Grinnell Land and Labrador. A depression which amounted to 1,000 feet in Grinnell Land, and affected all or nearly all of the Arctic archipelago and probably Greenland, must have greatly increased the water area in the north and caused a corresponding rise of temperature. This rise of temperature would undoubtedly cause a retreat of the glaciers, permitting the sea to extend much farther up the fjords than at present, and in the shells which the ice cap is now carrying from the valleys of the interior we have proof that such a retreat occurred. With the elevation of the land in the north again, a lower temperature prevailed and the ice recovered some of its former territory.

E. M. KINDLE.
YALE UNIVERSITY, December 8, 1896.

CURRENT NOTES ON PHYSIOGRAPHY.

THE SIERRA NEVADA.

'FURTHER Contributions to the Geology of the Sierra Nevada,' by H. W. Turner (17th Ann. Rept. U. S. G. S., 1896, 521–1076) contain many geographical items. Oroville table mountain is illustrated in an excellent plate. The deep, steep-sided canyons that have been cut into the uplifted mass of the range often have benches on their slopes, caused by landslides; these, with the falls in the streams and the inaccessible character of the canyons, may be taken as features of a youthful stage of geographical development. Associated with them as indications of recent uplift are occasional fault scarps, still steep and bare; one of these being shown in a plate. Of a little greater age are the fault-block lake basins, now drained by filling with sediments and cutting down at the outlet; Meadow valley being of this class. Mohawk valley, first holding a Pliocene lake in a fault basin, was afterwards obstructed in Pleistocene time by fragmental andesite flows. Much of the volcanic material, once broadly spread over the Sierra area and now greatly dissected since its regional uplift, is shown to be fragmental, coarse and fine, less or more stratified; it is compared in origin to the mud flows of modern volcanoes. The flows came from the crest of the range, and ran for fifty miles on the comparatively gentle slope of the then low-lying region. The 'hog-wallow' mounds on the valley plain and margin of the foothills are described and illustrated, but not definitely explained; they are one or two feet high, four to ten feet in diameter, and of the same pebbly soil as that on the intervening spaces.

NORTH CAROLINA AND ITS RESOURCES.

'NORTH CAROLINA and its Resources' is the title of a volume published by the State Board of Agriculture (Raleigh, 1896, 413 p., many plates), to which the geographer may refer with profit. The mountains, with their minerals, mines, forests, and attractive 'resorts;' the piedmont belt, with rich fields and great water powers, the coastal plain with its growing interests in truck farms and orchards, and the sounds with their fisheries, are all duly set forth. This report forms a fitting companion to a volume on 'South Carolina, resources and population, institutions and industries,' published some years ago by the State Board of Agriculture (Charleston, 1882).

The latter lacks illustrations, but has a soil map.

DUNGENESS FORELAND.

DR. F. P. GULLIVER continues his studies on Cuspate Forelands (Bull. Geol. Soc. Amer. VII., 1896), by a study of Dungeness foreland, on the southeastern coast of England, one of the best examples of its class; having read his paper on this subject at the Liverpool meeting of the British Association in 1896 (London Geogr. Journ. IX., 1897, 536–546). He gives a restoration of the initial shore line of the region, and outlines of successive stages in the growth of the foreland, whose cusp has grown eastward and outward during its enlargement. It now projects about ten miles into the channel from the original reentrant of the coast; near the apex the shingle ridges or 'fulls' indicate the lines of progressive growth with much clearness. It is noted that English sailors have recognized forms in other parts of the world similar to this home example, and have applied the home name to two widely separated forelands; one in Puget Sound, the other in the Strait of Magellan.

A FAULT LINE IN AFGHANISTAN

AN account of the southern borderland of Afghanistan by Captain McMahon (London Geogr. Journ., IX., 1897, 392–415) includes a description of a remarkable fault line, along which the topography of a growing displacement is visible. It was examined for a distance of 120 miles, on an almost direct course a little east of north, near the southeastern corner of Afghanistan; a well defined broad line of deep indentation, in many places as distinct as a deep railway cutting. It ran for a time along the border of the Registan plains, then obliquely traversed two mountain ranges, cutting the crest of one near its highest peak. Springs are common along it, and for this reason as well as because it forms a short cut across mountain spurs, the depression is commonly used as a thoroughfare. Igneous rocks form the country to the west, and sedimentaries lie to the east of the fault line. During the lifetime of the older natives, on the occasion of three severe earthquakes, deep fissures appeared along the depression, and the springs increased in volume. The line crosses a frontier railway near Chaman, beyond Quetta. A severe earthquake on December 20, 1892, opened a fissure where the fault crossed the track, distorting the rails, and lessening the distance between Quetta and Chaman by $2\frac{1}{2}$ feet. All the region is desert—bold, barren mountains, stony slopes, shifting dunes, alluvial and saline plains; many camels died in McMahon's trip across it.

W. M. DAVIS.

HARVARD UNIVERSITY.

SCIENTIFIC NOTES AND NEWS.

PROFESSOR ALFRED M. MAYER, the eminent physicist, died at Maplewood, N. J., on July 13th, aged sixty-one years.

ON the recommendation of Hon. Chas. D. Walcott, acting Assistant Secretary of the Smithsonian Institution, in charge of the U. S. National Museum, an important change has been made in the administration of the Museum. Three sections have been organized—a section of anthropology, a section of biology and a section of geology, each having a head curator with an annual salary of $3,500. Dr. W. H. Holmes has been appointed head curator of anthropology; Dr. Frederick W. True, head curator of biology, and Dr. George P. Merrill, head curator of geology. Dr. True and Dr. Merrill are already connected with the Museum, and it is expected that Dr. True will continue to act as the executive curator. Dr. Holmes leaves the Field Columbian Museum, Chicago, to accept this position, but was formerly connected with the U. S. Geological Survey and the Bureau of Ethnology.

THE Berlin Academy of Sciences made, at its last meeting, awards for scientific purposes

amounting to nearly $20,000. These awards were as follows: To Professor F. E. Schültze for the publication of *Das Tierreich* under the auspices of the Zoological Society, 35,000 M.; for the publication of the new edition of Kant's works, 25,000 M.; to Professor Engler for the publication of monographs on African botany, 2,000 M.; to Dr. G. Lindau for studies on Lichens, 900 M.; to Professor F. Frech for his geological studies, 1,500 M.; to Professor H. Hürthle for studies on muscles, 850 M.; to Professor R. Bonnet, for the preparation of a work on blood vessels, 800 M.; to Dr. Lühe, for investigations of the fauna of salt lakes of North Africa, 2,000 M.; to Dr. G. Brandes, for studies on Nemertina, 300 M.; to Dr. R. Hesse for investigations at Naples on the eyes of lower marine animals, 500 M.; to Professor E. Cohen for investigations of meteorites, 1,500 M., and to Dr. L. Wulff for experiments on artificial crystals, 1,500 M.

THE Academy of Sciences of Vienna celebrated, on May 30th, the fiftieth anniversary of its foundation. The government made this the occasion of increasing the annual subsidy of the Academy from 40,000 to 50,000 Fl.

THE German Zoological Society held its seventh annual meeting from the 9th to the 11th of June at Kiel. We learn from *Die Natur* that the program we have already announced was carried out, except that the president, Professor Bütschli, was unable to be present and the meeting was presided over by Professor J. B. Carus. There were thirty-seven members and thirteen guests in attendance. It was decided to hold the next annual meeting at Heidelberg at Whitsuntide.

THE eightieth meeting of the Swiss Scientific Association will be held at Engelberg from the 12th to the 15th of September. The place of meeting is in the midst of fine Alpine scenery a three and a-half hours' drive from the nearest railway station. American men of science are cordially invited to be present.

THE Anatomical Society, intended to be international in character, but chiefly supported by German anatomists, held its eleventh meeting at Genth from the 24th to the 27th of April, with fifty members and guests in attendance. The President, Professor Waldeyer, discussed anatomical nomenclature, and papers were presented by Professor O. Schültze, Professor v. Kölliker, Professor v. Bardeleben and others. The next meeting will be held at Kiel during April of next year.

THE preliminary program of the first International Congress of Mathematicians, to be held at Zurich from the 9th to the 11th of August, announces that in addition to two general meetings there will be six sectional meetings as follows: Arithmetic and algebra, analysis and theory of functions, geometry, mechanics and mathematical physics, astronomy and geodesy, history and bibliography. At the general sessions the following papers will be presented: 'On the relations between pure analysis and mathematical physics,' by Professor H. Poincaré; 'On the recent development of the general theory of analytical functions,' by Professor A. Huritz; 'On the teaching of higher mathematics,' by Professor F. Klein; On *Logica Mathematica*, by Professor G. Peano. The dues for the Congress are 25 fr., which sum includes the cost of the banquet and other entertainments.

THE second of the annual conversaziones of the Royal Society was held on June 16th. Demonstrations with illustrations were made by Mr. W. H. Preece on signalling through space without wires, and by Professor Lockyer on the arrangements of the 1896 eclipse expeditions. There were exhibits by Lord Kelvin of the electrical effects of uranium and of X-rays, by Professor S. P. Thompson, Mr. A. A. C. Swinton, Dr. John Macintyre, Dr. J. H. Gladstone and Mr. Walter Hibbert. There were also other interesting exhibits, both in the biological and in the physical sciences.

THE annual field meeting of the Indiana Academy of Science was held at Lafayette from the 26th to the 28th of May, with an attendance of forty members. Excursions were made in the surrounding regions, and an address was given by Professor Frederick Starr, his subject being 'Dress and Ornament.'

PROFESSOR A. C. GREENHILL, of Woolwich; Professor J. V. Jones, of Cardiff, and Professors John Perry and W. E. Ayrton, of London, have expressed their intention of being present at

the Detroit meeting of the A.A.A.S. and participating in the proceedings of the section for physics.

THE Albert medal of the Society of Arts, London, has been awarded to Mr. G. J. Symons for his services to meteorology.

THE Senckenberg Society of Natural History at Frankfort has awarded the Sommering prize, consisting of a medal and 500 M., to Professor Gustav Born, of Breslau, for his researches on the growth of the larvæ of amphibia.

THE death is announced of Dr. P. Schützenberger, professor of chemistry at the Collège de France, at the age of sixty-seven years. He had been since 1888 member of the Paris Academy of Sciences and had made important contributions to organic chemistry.

WE also regret to announce the following deaths: Dr. Alfred Moquart, professor of anatomy at Brussels, on June 5th; Professor Martin Wilckens, of the Agricultural School of Vienna, on June 10th, at the age of sixty-four years; Count Victor Trevisan di S. Leon, the cryptogamist, in Milan, on April 8th, and Frau Dr. Vera Bogdanowskaja-Popoff, on May 8th, as the result of an explosion while carrying on chemical experiments.

DURING the Moscow meeting of the International Medical Congress a statue to the eminent surgeon Pirogof will be unveiled. The sum of 12,000 roubles (about $6,000) has been collected by public subscription for the statue, the sculptor of which is Mr. V. R. Sherwood.

M. HATT and Professor de Lapparent have been elected members of the Paris Academy of Sciences.

MR. F. D. GODMAN has been elected President of the British Ornithologists' Union.

PROFESSOR P. ROUSSELOT, of the École des Hautes Études, Paris, has been appointed director of the laboratory for experimental phonetics, the establishment of which under the Collège de France we recently announced.

MR. HENRY L. BRYAN has been appointed, by the trustees of the Philadelphia Commercial Museum, Secretary of the Museum.

THE U. S. Civil Service Commission announces a competitive examination, on August 9th, for the purpose of establishing a register from which certification may be made to the position of Examiner, Mint Bureau, Treasury Department, at a salary of $2,500 per annum. The duties of the position comprise the inspection and supervision of all the machinery installed in different U. S. mints throughout the country. Applicants should be graduates of recognized technical schools giving courses in mechanical engineering, or should, in lieu of this, have very broad training and experience along the lines of mechanical engineering.

PRESIDENT JORDAN has passed through Seattle on his way to Alaska. He is accompanied by Professor Wood, of Stanford University.

MR. H. W. TURNER, of the U. S. Geological Survey, and Professor John C. Branner, of Stanford University, are engaged in exploring in the region of the Yosemite and Hetch-hetchy Valleys and the adjacent mountains.

MR. R. W. PORTER and Mr. A. V. Shand, who are accompanying Lieut. Peary on his present expedition, expect to spend the winter in Baffin Land making ethnological and zoological studies and collections. They expect, in the summer of 1898, to explore the country northward and to return on a whaling ship from Cumberland Sound to Aberdeen.

THE men of science who will embark on the 'Belgica' on its approaching expedition to the Antarctic regions will be as follows: The captain, M. A. de Gerlache, geology, meteorology and oceanography; M. Arctowski, terrestrial magnetism and physics; M. Danes, zoology, and M. Racovitza. The crew have already embarked in Norway and it is expected that the steamship will leave Antwerp on the 25th of the present month. The men of science expect to spend the Antarctic winter in Victoria Land, while the steamship will go to Melbourne to renew its stores.

PROFESSOR F. A. STARR has returned to the University of Chicago, from an expedition to New Mexico, having explored one of the mesas and one of the caves of the Cochitas, and having secured plaster casts of the busts of a number of Pueblo Indians.

THE Canadian Pacific Railway Company will sell first-class return tickets to members of the British Association from Toronto to the Pacific Coast, at a rate varying between $61.80 and $70.30, according to the route selected. This is less than a single fare and the tickets are available from July 1st to October 1st.

DARWIN'S family have presented to Cambridge University the geological specimens found during the voyage of the 'Beagle' and a series of slides used in the preparation of his monograph on the Cirripedia. The former has been placed in the Museum of Geology, the latter in the Museum of Zoology.

IN celebration of the Cabot quatercentenary the foundation stone of a memorial tower to be erected on Brandon Hill, Bristol, was laid on June 25th. The tower will be 100 feet in height, squarely built with emblematic panels. Some of the bas-reliefs will be contributed by an American committee, the President of which is Mr. Bayard, lately Ambassador to Great Britain. Lord Dufferin made a speech describing what little is known of Cabot and the adventurous voyage of the 'Matthew' and its importance for the extension of Anglo-Saxon civilization.

THE Geological Society of Portugal opened on July 8th the new Geographical Institute founded at Lisbon in commemoration of the 400th anniversary of Vasco da Gama's departure for the Indies.

THE Academy of Medicine of Paris has received a legacy of 15,000 fr. from Mme. Clarens for the foundation of an annual prize.

THE scientific library of the late Sir G. Humphrey, professor of surgery at Cambridge, has been presented by Mrs. Humphrey to the surgical department of the University.

THE town of Middletown, N. Y., receives, by the will of the late Mrs. S. Maretta Thrall, $30,000 for a public library. She had already given the town a hospital and a park, the value of her gifts aggregating $80,000.

THE Board of Education of the City of New York has adopted a resolution providing for the employment of oculists to report upon the best colors to be used in painting and decorating schoolrooms with reference to their effects on the eyesight of children.

REPRESENTATIVE LACEY, of Iowa, has introduced a bill in the House of Representatives providing that the name of the Fish Commission shall be changed to the Commission of Fish, Fisheries and Birds. It is proposed that the Commission shall extend its jurisdiction to provide for the propagation, distribution and restoration of game and other wild birds of the United States. It is not likely that this change will be made, as wild birds and mammals are already provided for under the Department of Agriculture, and any extension of the work should be developed under that Department.

BEGINNING with the number for July, the *Physical Review* will be published by the Macmillan Company in two volumes annually. These volumes will begin in January and July respectively, and will each contain about five numbers.

Natural Science for July, now published by J. M. Dent & Co., and printed by Turnbull & Spears with improved typography and better paper, contains articles by Professor Bashford Dean on the Hopkins Seaside Laboratory of Stanford University, and by Dr. P. L. Sclater on 'The Proposed Zoological Park in New York.' *Natural Science* should have given credit to *Nature* for the queer note to the effect that Professor Putnam and Dr. Boas have started on a six years' expedition to study the relation of the American Races to those of Asia and Africa. "They will proceed up the northwest coast of North America, cross Behring Strait, and so pass down through eastern Siberia into China, and thence along the Indian Ocean to Egypt."

THE presidential address by Dr. G. W. Hill before the American Mathematical Society in 1895, printed in the issue of this JOURNAL for March 6, 1896, is published in the current number of the *Revue Scientifique*. It is credited as a presidential address before *l'Association scientifique americaine*.

PROFESSOR RÖNTGEN has contributed to the *Berichte* of the Berlin Academy an account of further observations on the properties of the X-rays. He has observed that the rays emanate from the irradiated air in all directions so

that if the rays were visible the appearance would be that of a room filled with smoke and lighted up by a candle. When a plate impervious to the rays is placed between a fluorescent screen and a source of the rays the platinocyanide of barium nevertheless becomes luminous, and this luminosity is visible even when the screen lies directly upon the plate. If, however, the screen placed on the plate is covered by a cylinder of lead 0.1 cm. in thickness surrounding the fluorescent screen the fluorescence disappears. Professor Röntgen has further been able to measure the intensity of the rays and to study the influences on which this depends. Dr. Brandes' observations that the X-rays may be made visible, presumably by causing fluorescence of the retina, are confirmed. Professor Röntgen sums up the present state of our knowledge in regard to the rays as follows: (1) The rays proceeding from the discharging apparatus are a mixture of rays varyin absorbability and intensity. (2) The composition depends chiefly on the duration of the discharging current. (3) Different bodies absorb different kinds of rays. (4) The X-rays are produced by the cathode rays and the phenomena of both are probably of the same nature.

THE compilation of the statistics of coal production in the United States in 1896, which has just been completed by Statistician E. W. Parker, of the U. S. Geological Survey, shows that the product in 1896 was 190,639,959 short tons, valued at $195,557,649, against 193,117,-530 short tons, valued at $197,799,043 in 1895, a decrease of 2,477,571 short tons in amount, and of $2,241,394 in value. The decrease in product was entirely in that of Pennsylvania anthracite. The output of bituminous coal shows an increase of about one and three-quarters million tons. The anthracite product of Pennsylvania decreased nearly four and a quarter million tons. It is a notable feature, however, that there was a decrease in the value of the bituminous product of over $1,600,-000, notwithstanding the increased output, and that there was a comparative increase in the value of anthracite, although, on account of the smaller production, it did not equal the value in 1895. The average price obtained for anthracite at the mines increased from $1.41 in 1895 to $1.51 in 1896. The average price for bituminous declined from 86 cents to 83 cents.

AT the coming International Leprosy Conference, to be held in Berlin on October 11th, Dr. Hutchinson, of London, will report on alimentation and leprosy; Professor Virchow on the pathological anatomy of leprosy; Dr. Neisser, of Breslau, on its origin; Dr. Bernier, of Paris, on its etiology, and Professor Koch will discuss the question of its infectiousness.

WE learn from *Natural Science* that the Natural History Museum of Halifax, which was handed over to the County Borough Council about eighteen months ago by the Literary and Philosophical Society, has now found a permanent home in the old mansion named Belle Vue. The geological and botanical collections are very extensive and valuable, but zoology is as yet very imperfectly represented. The herbarium has lately been much enriched by the fine Gibson collection of British plants, the gift of Lady Trevelyan. The Curator, Mr. Arthur Crabtree, is making an attempt to render the Museum of general educational value by adequate labelling, and wishes to secure a competent committee of management to direct and second his efforts.

UNIVERSITY AND EDUCATIONAL NEWS.

THE Supreme Court of New York State issued on July 6th its final decision in the Fayerweather will case. The executors were required to distribute within ten days the three million dollars in question to the twenty colleges to which they were bequeathed.

THE only colleges so far as we have noticed which have this year given the Ph. D. degree *causa honoris* are Union, Dartmouth and Tufts. These colleges have acted unwisely and Union College, as we understand it, illegally.

PROFESSOR WILLIAM A. ROGERS, who recently accepted the Babcock professorship of physics in Alfred University at Alfred, N. Y., delivered the principal address at the laying of the corner-stone of the Babcock Hall of Physics at Alfred on June 22d. The hall is named after the late George H. Babcock, of Plainfield, N. J., who left $100,000 to Alfred University.

AT the University of Indiana A. L. Foley, Ph. D. (Cornell), has been elected professor of physics; R. J. Aley, Ph. D. (Pennsylvania), professor of mathematics; E. B. Copeland, of the University of Wisconsin, assistant professor of botany, and E. B. Bryan, assistant professor of pedagogy.

HENRY C. MINTON, of San Francisco, was elected President of Centre College this week.

DR. G. J. PIERCE has been elected assistant professor of botany in Stanford University.

IN the newly organized high schools of New York City, as the result of a competitive examination, there have been appointed as first assistants, at a salary of $3000, Mr. Frank Rollins, chemistry; Mr. R. H. Cornish, physics, and Mr. E. W. Sampson, physical geography.

THE University of Strasburg has celebrated, by fêtes lasting several days, the 25th anniversary of its foundation.

PROFESSOR W. TH. ENGELMANN, of the University of Utrecht, has been offered the chair of physiology at Berlin, vacant by the death of Du Bois-Reymond, but it is stated that he will not accept. The position had previously been twice declined.

DR. JAEGER and Dr. Brodhun have been appointed professors at The Reichsanstalt at Charlottenburg; Dr. Ignaz Zakezewski has been made full professor of experimental physics at the University at Lemburg, and Dr. H. Finger, of Giessen, has been appointed assistant professor of organic chemistry at the Polytechnic Institute in Darmstadt.

DISCUSSION AND CORRESPONDENCE.

A BRILLIANT METEOR.

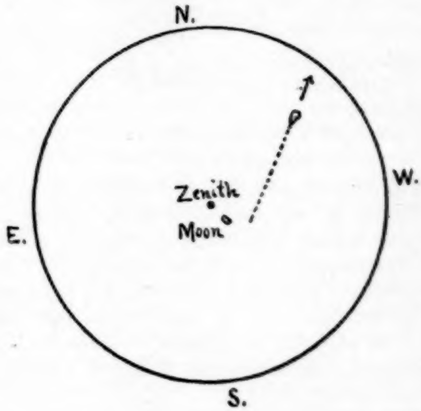
TO THE EDITOR OF SCIENCE: On June 22d a brilliant meteor was observed in broad daylight passing over Cape Breton Island, Nova Scotia. In order that some permanent record of the fact may exist, I beg to forward for publication a letter received from Mr. George Kennan, who was an eye-witness of the occurrence.

ALEXANDER GRAHAM BELL.

BEINN BHREAGH, NEAR BADDECK, C. B.,
NOVA SCOTIA, July 1, 1897.

Letter from Mr. George Kennan.

MY DEAR MR. BELL: I will gladly give you all the information I can in regard to the meteor. Between half-past eight and nine o'clock, Tuesday morning, June 22d, as I was going into my garden to work, a very large and extraordinarily bright meteor suddenly made its appearance nearly southwest of the zenith, at a height of about 70 degrees from the horizon and almost directly under the old waning moon. I happened at that time to be looking upward and westward, and I think I saw it at its place of origin—that is, at the point where it first became visible. It was not a mere point of light, like a brilliant star, but seemed to have a large, well-defined disc, resembling in shape a somewhat elongated and almost inverted balloon with its top or larger end foremost—that is, turned in the direction of its fall. It came into



the field of vision so near the waning moon that I was able to compare the one with the other in point of size, and the impression made upon my mind was that the disc of the meteor was nearly as large as the filled-out circle of the moon would have been. Of course, the eye unconsciously exaggerates the size of a brilliant object, and it probably did so in this case, but such was the impression made upon me, and I give it for what it may be worth. I don't think the meteor had any decided color. At any rate, all that I noticed was its extraordinary brilliancy. If it had been decidedly green, red,

or yellow, I think I should have become conscious of the fact. It seemed to be followed by a faint line of light about half as long as its own body. From a point about 20 degrees southwest of the zenith it fell rather swiftly in a direction that would have brought it to the horizon at a point very nearly northwest (magnetic) of my position. I was unable, however, to follow it all the way to the horizon on account of the trees between my house and your cottage behind which it disappeared without having lost either its shape or its brightness. The time occupied by its fall was not more, I think, than three seconds. If you will hold the accompanying diagram above your head like a celestial chart and look up at it, facing the west, you will get an approximate idea of of the meteor's course as it appeared to me. The sun was shining brightly, but it did not overcome the brilliancy of the aerolite.

Sincerely yours,

GEORGE KENNAN.

BRETON COTTAGE, BADDECK, C. B.,
NOVA SCOTIA, June 26, 1897.

SCIENTIFIC LITERATURE.

WHAT ARE STIPULES?

'THE Nature and Origin of Stipules,' by A. A. Tyler, A. M. Presented to the Faculty of Pure Science of Columbia University in partial requirement for the degree of Doctor of Philosophy. Annals of the New York Academy of Sciences, Vol. X., New York, 1897, pp. 1–49, pl. i.–iii. Also separate: Contributions from the Department of Botany of Columbia University, No. 119.

This is, without doubt, the most considerable contribution that has been made to the vexed question of the nature of stipules; at least it is safe to say that it comes much nearer to a solution of that question than anything that has hitherto been brought forward. Although assuredly not the last word that will be said on the subject, nevertheless the light that had already been shed upon it by a long train of previous investigations placed the author in a position to treat it from an advanced standpoint.

Nearly half the paper, and that the first half, is devoted to summing up, in chronological order, the views that have been expressed and the conclusions that have been reached; but the paper is by no means a mere literary effort. It is itself the result of a series of special researches on the part of the author. Indeed, it may be looked upon as a new departure, since his investigations have proceeded from an almost entirely different standpoint from those of previous authors. He has made use of their labors and failures rather as a means of warning than as guides to his work.

Probably the most suggestive results that had been reached were those which, within the past decade, have been furnished by paleontology, and while he has singularly omitted to mention the researches of Saporta and Marion,* he has not left out of account those that have been made in America.† It is not too much to say that these paleontological discoveries have added more to our acquaintance with the true nature of stipules than the combined morphological studies of previous authors. If I do not mistake, it was from attention to paleontological considerations as thus brought out, that Mr. Tyler was led to adopt the method of his thesis, a method which had been wholly neglected hitherto, and yet the only one that seems to promise ultimate success in the solution of the problem.

The earliest of the above mentioned papers called attention to certain remarkable basilar expansions that occur in leaves of *Platanus basilobata*, a fossil species from the Fort Union

*Evolution du Règne Végétal, Pt. II., Phanérogames. Paris, 1885. See especially Vol. I., pp. 201–223; Vol. II., pp. 9–44.

† The Paleontologic History of the Genus Platanus by Lester F. Ward; Proc. U. S. Nat. Mus., Vol. XI., 1888, pp. 39–42, pl. xvii.–xxii. Origin of the Plane-Trees, by Lester F. Ward; Am. Nat., Vol. XXIV., September, 1890, pp. 797–810, pl. xxviii. Flora of the Dakota Group, by Leo Lesquereux; Monogr. U. S. Geol. Surv., Vol. XVII., pp. 65, 231, 232, 254. Wing-like Appendages on the Petioles of *Liriophyllum populoides* Lesq. and *Liriodendron alatum* Newb., with Descriptions of the Latter, by Arthur Hollick; Bull. Torr. Bot. Club, Vol. XXI., No. 11, November 24, 1894, pp. 467–471, pl. ccxx., ccxxi. Appendages to the Petioles of Liriodendra, by Arthur Hollick; Bull. Torr. Bot. Club, Vol. XXIII., No. 6, June, 1896, pp. 249–250, pl. cclxix., cclxx.

group, which was compared with another fossil species, *P. appendiculata* of Lesquereux, in which somewhat similar basilar expansions occur, but in this case separate from the main blade. The latter species comes from the Auriferous gravels of California, a much later formation, and it was argued that these two cases indicate a gradual separation of these lobes from the blade as having taken place in the progress of development. It was further shown that the living American species, *P. occidentalis*, sometimes has a small expansion at the base, through which the petiole passes, and that other cases may be found on young shoots in which these lobes are distinct from the blade.

The second paper cited, though chiefly a criticism of a memoir by Jankó, on the leaves of Platanus, discusses the subject of basilar expansions with additional illustrations.

Dr. Hollick's papers deal with an almost similar condition of things in the fossil species of Liriodendron. Both of these genera belong to what are known as waning types, and their present foliage has reached its highest state of development.

These and other facts that have been from time to time coming to light had begun to imbue botanists who had given attention to the subject with the general idea that stipules are morphologically portions of the leaf that have been gradually separated from the existing leaf blade through a process of development going on under the influence of the great principle of advantage in biology, which relegates to the domain of vestiges or obsolescent organs everything that has ceased to benefit the organism, a process which has as its ultimate result the complete extinction of such organs, and there is no doubt that in many cases modern exstipulate leaves have once had stipules and lost them, although it is also true, as Mr. Tyler shows, that in other cases, especially those of sessile leaves, no stipules have ever existed.

In 1894 Mr. Morong, in treating the genus Smilax,* said: "Most of the species climb upon other shrubs and trees by means of a pair of tendrils which grow at the summit of a stipular wing on each side of the petiole, often not developing till the stem is several years of age;" to which he adds in a footnote: "De Candolle regards this appendage as more in the nature of a modified leaf segment or leaflet than a stipule, but it seems to me that a stipule is nothing else than a leaflet at the base of a petiole." Mr. Tyler does not note this statement of Mr. Morong in his bibliographic summary, but it is characteristic both of Mr. Morong's keen insight into such matters and also of the general drift of botanical thought on the subject.

*Bull. Torr. Bot. Club, Vol. XXI., No. 9, September 29, 1894, p. 420.

In the paper before us there are brought out into clear light at least three distinct and highly important facts. The first relates to method. It had long been felt that the great need in botany was the study of plants from the embryological standpoint in some such way as animals have been studied with such remarkable results. The two great sources of our knowledge of development in both kingdoms are, first, paleontology, and second, embryology. Both of these had been almost totally neglected by botanists until within recent years. Something, it is true, had been done along the more general lines of plant development from the paleontological side, but scarcely anything in connection with the transformation that leaves undergo, and the few papers above quoted constitute practically all that has been done in this line. A number of attempts have, however, been made to approach the vegetable kingdom from the embryological point of view. But the seed being regarded in a certain sense as the homologue of the egg, most of these attempts have been devoted to the study of the seed and of seedlings, the great work of Sir John Lubbock constituting the most exhaustive of these efforts.

Mr. Tyler has shown in the present paper that the study of plant embryology should not proceed from a consideration of seeds and their development, but of buds, and that while botanists have been mainly seeking for light in the difficult study of seed embryos, the true source of such knowledge is the much more accessible phenomena of bud development. The figures that he has given abundantly demonstrate this truth, and henceforth there can be no doubt that botanists generally will proceed according to this method and that the

natural history of the leaf will soon be fully known.

The second lesson which Mr. Tyler's paper enforces is the one which paleontology, as we have seen, had already taught. What the leaf struggles to secure is the maximum amount of light and air. In this effort it constantly tends to stretch out as far a possible from the stem. The proximal portion, which is most shaded, is then sacrificed to the distal portion, and the leaf is differentiated into petiole and blade. But in this process various stages occur. Those parts which are of any advantage to the plant are in part retained, and the parts sacrificed are selected in the exact measure of their failure to benefit the plant.

Some may regard the principle of adaptation for securing the maximum light and air as inadequate to explain these modifications of the leaf, but it is this principle that determines not only the form but also the arrangement of leaves, and when we remember that opposite leaves are also decussate, that in whorled leaves those of one whorl stand over the intervals between those of the next whorl below, and that even in plants with alternate leaves the phyllotaxy is so adjusted as to secure the longest interval between one leaf and the next one that must stand directly over it, we not only see with what rigorous exactness this principle works, but also what apparently trifling differences in advantageousness are seized upon and made to count in producing manifest effects.

The least useful portion of a leaf is not that at the very base, but that which is some distance from the base, and even this may be partially retained as a wing to strengthen the leafstalk. The portion at the base is often preserved in one form or another, and we have seen, in the fossil and living species considered, how this may vary in the process of evolution, but in the most highly developed of our living flora, where it is retained at all, it is usually in the form of stipules, which have all conceivable shapes and differ in all degrees in their permanence, some being appropriated to other uses according to the law of vicarious function. Others are persistent as small organs of different forms. Still others are deciduous at varying stages in the growth of the leaf, some, as Mr. Tyler shows, never being seen except on dissecting the bud. The last stage in this process is their complete atrophy and the resultant wholly exstipulate leaf.

The third lesson that we learn from Mr. Tyler's studies is that monocotyledonous plants constitute an early stage in the process of leaf development. This is what would have been naturally supposed, but there has been a tendency of late to cast doubts upon the position of the monocotyledons and to maintain that they are as highly developed and that they have been as late in their appearance in geologic time as the dicotyledonous angiosperms. It must be admitted that the paleobotanists have been the ones who have chiefly taken this view. This has been due to the exceedingly meager representation which the monocotyledons have in the fossil floras of the globe, and especially to the natural doubts which have arisen as to the botanical character of most of the fossil forms that have been regarded as monocotyledons by certain authors. The Yuccites of Schimper, from the Lower Trias, as also his genus *Æthophyllum*, which he did not himself refer to that class, but which others have naturally regarded as a monocotyledonous plant, cannot certainly be claimed as ancestral monocotyledons, although the proof to the contrary is equally wanting. Scarcely anything that has been discovered in the great Jurassic floras of the world has even been called monocotyledonous, and very little that is certainly such occurs even in the Cretaceous. It is, therefore, been held by some that this class of plants first made its appearance with the palms of the Eocene, but so rich and varied is this Eocene palm flora that it presents a case analogous to that which until recently was offered by the dicotyledonous floras of the Middle Cretaceous, and requires the violent assumption that a great group of plants suddenly burst in upon the world and attained all at once a high state of development in widely separated regions. This assumption is now thoroughly disproved in regard to the dicotyledons by the discovery of early embryonic types at a much earlier age, naturally leading up to the higher types referred to.

The monocotyledons, from their very nature, are the least adapted of all forms of vegetable

life to be preserved in the fossil state, representing, as Saporta and Marion show, and as Mr. Tyler's researches fully bear out, the primitive form of leaf development, which consists simply in setting apart a portion of the growing plant to serve the purposes of leaves, consisting of more or less broad and elongated blades, usually embracing the stem and tapering gradually to a point, with the leaf bundles continued in straight lines parallel to each other throughout their entire length. They are, therefore, broadest at the base and least adapted to securing the ultimate purpose of leaves already mentioned, viz., the maximum amount of light and air. The process of leaf development began with this condition, and many of the forms in which the cotyledon is still single have acquired a blade, as, for example, many species of *Potamogeton*, *Smilax*, *Dioscorea*, etc. In *Smilax* and some other genera true stipules have been developed, along with the tendency towards their differentiation into tendrils and other useful organs.

An important obstacle to the preservation of monocotyledonous leaves in the fossil state is the absence in them of any definite joint or natural point of separation of the leaf from the stem, which is one of the earliest results in the process of leaf evolution, also involving the principle of the renewal of leaves at annual or other fixed periods, which has practically resulted in the indefinite multiplication of the leaves produced, increasing the chances of their preservation by the whole number of such renewals. The only chance for an ordinary monocotyledonous plant to become entombed and preserved in the fossil state is that the locality in which it grows shall become somewhat rapidly covered up, burying the entire plant so quickly that it cannot decay during the process. This, as anyone can see, must be an exceedingly rare occurrence. Still, there is no doubt that a large amount of monocotyledonous vegetation growing in bogs and marshes in estuarine regions that are slowly subsiding under the weight of materials brought down the streams, and which also aid in covering them up, has been, in fact, preserved in a very imperfect way, and many vague and puzzling objects occur in all collections made from such localities. They are found throughout the Mesozoic, in the form of short culm-like segments and imperfect bits of leaves so badly macerated that they are neglected by those who determine such collections. It is rarely possible to say what form of plant they really represent, and yet it is often clear that these remains belong to certain glumaceous forms, grasses, sedges, rushes, etc. Saporta, in his work on the Mesozoic of Portugal, described and figured, under the name of Poacites, quite a number of these forms from the Neo-Jurassic to the Albian, or through the Upper Jurassic and entire Lower Cretaceous. Others have been called Cyperites, Zosterites, Bambusium, etc. Numerous small seeds are also constantly occurring, which are for the most part unnamed or given such names as do not indicate their botanical affinities. Many of these probably belong to monocotyledonous plants.

Mr. Tyler's paper, with all its excellencies, conveys the impression of an unfinished production. One would say that in his hurry to use it as a thesis he had been obliged to close it up abruptly. Its most serious defect is the want of careful descriptions of the plates and figures explained in their numerical order for the convenient use of the reader. This condition of the paper suggests the probability that the writer has much additional material, and inspires the hope that he may have entered upon a much more extended and exhaustive series of observations along these suggestive lines.

LESTER F. WARD.

On the Genera of Rodents: An Attempt to bring up to Date the current Arrangement of the Order. By OLDFIELD THOMAS, F.Z.S. Proc. Zool. Soc., London, 1896, pp. 1012–1028. Issued April, 1897.

The order Rodentia offers peculiar difficulties to the student, both on account of the number of its species and the great variety of forms which it includes. The satisfactory arrangement of the thousand or more species now known is no easy matter, as shown by the attempts of several authors, notably Waterhouse in 1839–48, Gervais in 1848–53, Brandt in 1855, Lilljeborg in 1866, Gill in 1872, and Alston in 1876. During the last ten years more progress has been made in the study of mammals than

in any previous quarter of a century. New methods of collecting, and more thorough exploration of regions previously little known, have brought to light a host of new forms and furnished material for studies which have thrown new light on the relationship of many groups. More careful examination of the literature has likewise necessitated many changes in nomenclature. Thus it is not surprising that Alston's classification, which has been generally adopted during the last 20 years, should have become somewhat antiquated.

In many respects Mr. Thomas is peculiarly fitted for the task of 'bringing the arrangement of the order up to date.' As curator of mammals in the British Museum he has constant access to a collection of rodents, which includes representatives of all but 15 of the existing genera and is unrivalled in the possession of a large number of types. Perhaps no other zoologist is personally familiar with more species or has a better general knowledge of the Rodentia than Mr. Thomas. He has also done much towards placing the nomenclature on a sound basis and has kept fully abreast of recent morphological work.

Although his paper comprises only 16 pages, it is an unusually important contribution to the literature of mammals and its value is not to be measured merely by its length. Unlike Alston's paper, it contains no diagnoses, and is therefore merely a list of genera arranged by families and subfamilies. It is intended mainly as a convenient reference list for museum curators and writers who have neither the time nor the inclination to work out the relationships of genera. Its object is threefold, since it gives: (1) the position and sequence of the genera in their respective subfamilies; (2) the earliest available name for each genus, and (3) a reference to the original description. The results of the investigation here presented necessitated not only a study of the genera and families, but the selection of the proper name from a host of synonyms for each of the 160 groups which are considered worthy of generic rank.

It would have been very desirable if the list had included extinct as well as living forms and had been extended to subgenera, thus forming a complete conspectus of the order. In limiting it to living mammals the author restricted himself to forms with which he is personally familiar, and by omitting subgenera avoided a vast amount of work which would have inevitably delayed the appearance of the list.

The changes which have been made in the classification of Rodents during the last 25 years can be most clearly brought out by comparing the lists of Gill, Alston and Thomas, but in so doing it should be remembered that Thomas follows Alston as closely as possible in the arrangement of the higher groups. Gill in 1872 recognized 9 superfamilies, 20 families, 16 subfamilies, but mentioned no genera; Alston in 1876 gave 3 superfamilies, 18 families, 23 subfamilies and 100 genera; Thomas now admits 5 superfamilies, 21 families, 27 subfamilies and 161 genera.* The increase in the present list is due to elevating the Bathyergidæ, Heteromyidæ, Erethizontidæ and Pedetidæ to the rank of families, and reducing the Lophiomyidæ to a subfamily of Muridæ. About one half the additional genera are 'new discoveries' and the remainder are due to the breaking-up of old genera.

Recent writers divide the Rodentia into two suborders: Simplicidentata and Duplicidentata; and most of them have followed Alston's tripartite division of the Simplicidentata into Sciuromorpha, Myomorpha and Hystricomorpha. This simple arrangement has not proved satisfactory, since some of the outlying genera will not fit into either group. To meet this difficulty Thomas has added two groups: Anomaluri and Aplodontiæ, making 5 subdivisions of superfamily value, thus to some extent following Gill. But in the attempt to retain Alston's higher groups with the termination *morpha* and at the same time to distinguish others of lower rank he has introduced two subdivisions between family and suborder. The names adopted are unfortunate, since the terminations are not distinctive, having been used by different authors for divisions varying in rank from superfamilies to subgenera. It would be simpler to

*Only 159 are mentioned, but *Fiber* is inadvertently omitted, and *Chilomys* has been proposed since this paper was printed. Beside these *Sigmodontomys* and *Zygodontomys* have recently been described by Dr. J. A. Allen.

adopt Gill's termination *oidea* for all these groups, although such a course would reduce the Sciuromorpha to the same rank as the Anomaluridæ. The relation of Thomas' superfamilies to those of Gill and Alston is as follows :

Anomaluri=Alston's Anomaluridæ=Gill's Anomaluroidea.

Sciuromorpha=Alston's Sciuromorphia (minus the Anomaluridæ and Aplodontiæ=Gill's Sciuroidea+Castoroidea.

Aplodontiæ=Alston's Haplodontidæ = Gill's Haploodontoidea.

Myomorpha=Alston's Myomorpha=Gill's Lophiomyoidea + Myoidea + Myoxoidea+Saccomyoidea.

Hystricomorpha=Alston's Hystricomorpha =Gill's Hystricoidea.

Two of Thomas' subfamily names, Loncherinæ and Sigmodontinæ are open to question on grounds of priority. The Loncherinæ were separated as a distinct group by Burmeister in 1854, but Gray had previously recognized the subfamily Echimyna in 1825, and Gill adopted the name in the form Echimyinæ in 1872. *Echimys* and *Loncheres* both belong to the same subfamily, and Echimyinæ besides being more generally used than Loncherinæ has several years' priority. Sigmodontinæ must give way to the well-known term Cricetinæ, the change having been made through a misapprehension as to the validity of the generic name *Cricetus*. As will be shown further on, there is no reason for rejecting *Cricetus* or the subfamily of which it is the type.

The instability of generic names is strikingly exemplified by this list. No less than one-eighth of the genera have been 'changed' during recent years, and in the attempt to find names which have unquestioned priority and are not preoccupied, the author has introduced unfamiliar terms for about 10 per cent. of the genera. In all such cases, however, the commonly accepted designations are added in brackets. But it may be questioned whether he has really carried this work far enough, for several of the names left undisturbed are open to objection.

Arctomys, which is usually credited to Schreber, 1792, can be traced back to 1780, but even with this early date it will probably have to give way to *Marmota* Blumenbach, 1779. The latter appeared in the first edition of the Handbuch d. Naturgeschichte—a rare volume, which is not accessible at present, and hence it is impossible to ascertain what species were originally placed in the genus.*

Hamster Lacépède appeared in 1801, whereas *Cricetus* was described by Cuvier in 1798, although not named until 1800. It was, however, defined by Kerr in 1792, and therefore antedates *Hamster* by not less than nine years. This is an excellent illustration of the importance of ascertaining the first publication of a name. Quoting *Cricetus* from Cuvier, Thomas assumes it to date from 1817 and rejects it in favor of *Hamster*, 1801. Had he found Cuvier's first use of the *Cricetus* in 1800 he would have avoided changing a name which must now be restored.

Cœlogenus Cuvier, 1807, appeared six years later than Lacépède's *Agouti*, the latter having been published in the Mem. de l'Institut, Paris, III., p. 494, 1801. As both of these genera were based on the same animal, *Agouti paca* (Linn.) is the proper name for the common paca.

Lagostomus Brookes, 1829, is antedated by *Vizcacia* Schinz, 1824,† and should be replaced by it.

Ellobius Fischer, 1814, may be considered untenable by some zoologists because of the prior use of *Ellobium* by Bolten in 1798 for a genus of mollusks,‡ but those who reject *Ellobius* must find a substitute for it, probably in *Chthonoergus* Nordmann, 1839.

The references to the original description of each genus in the list will be found very useful, but in a number of cases the names were actually published from one to twelve years

* In the 7th edition of the same work, published in 1803, Blumenbach included *Marmota alpina*, *M. cricetus*, *M. lemmus*, *M. typhlus* and *M. capensis*.

† See SCIENCE, New Ser., VI., July 2, 1897, pp. 21-22.

‡ Museum Boltenianum, 1798. See Adams, Gen. Recent Moll., II., 1858, p. 237. Both names are derived from the same Greek word, the neuter noun ἐλλόβιον, an ear ring. Should it be desirable to place the mammal genus in a separate subfamily, as Gill has already done, the designation Ellobiinæ becomes identical with that in use for a subfamily of mollusks.

earlier than here indicated. The author could hardly be expected to verify all his references and had he done so the result might not have justified the labor. Nevertheless, the failure to find the original description may result in an error which will necessitate a change in the type of a group or may even lead to the rejection of a valid current name as in the case of *Cricetus*. As Mr. Thomas has evidently given merely the references usually quoted by authors, the remarks on this part of the paper should be regarded as supplementary notes rather than criticisms. For the benefit of those who may use the list, the earliest references are given below for genera which were published before the dates assigned by Thomas:*

9. *Arctomys* Schreber, Säugthiere, plates CCVII.-CCIX., 1780, text IV., pp. 721–743, 1782 (not '1792').

16. [*Myoxus* Schreber, Säugthiere, IV., plates CCXXV. A–B, CCXXVII., 1782, text IV., pp. 824–831, 1787] (not '1792').

19. *Graphiurus* F. Cuvier, Proc. Zool. Soc. London, p. 5, July, 1838 (not '1845').

31. *Rhombomys* Wagner, Gelehrte Anzeige K. Bay. Akad. Wiss. München, XII., pp. 421, 429, 433, March, 1841 (not '1843').

35. *Dendromus* A. Smith, Zool. Journ., IV., pp. 438–439, Jan.-May, 1829 (not '1834').

61. *Cricetus* Kerr, Anim. Kingd., I, Mamm., pp. 42, 242–246, 1792 (not Cuvier, '1817').

72. *Rhipidomys* (Wagner), Tschudi in Wiegm. Archiv. 1844, I., p. 252 (not '1845').

96. [*Cuniculus* Wagler, Nat. Syst. Amphibien, p. 21, 1830] (not '1832').

— *Fiber* G. Cuvier (Tableau Elem. d'Hist. Nat. Anim., p. 141, 1798), Lecons d'Anat. Comp., I., Tabl. 1, 1800.

100. *Tachyoryctes* Rüppell, Neue Wirbelthiere z. Fauna von Abyssinien, Säugth., pp. 35–37, Taf. 12, 1835.

108. *Heteromys* Desmarest, Nouv. Dict. d' Hist. Nat., 2d ed., XIV., pp. 180–181, 1817 (not '1822').

115. *Dipus* Schreber, Säugthiere, pls. CCXXVIII.-CCXXXII., 1782, text IV., pp. 842–861, 1788–89 (not 'Gmelin, 1788').†

137. *Echimys* Cuvier, Nouv. Bull. Soc. Philom., p. 394, Sept., 1809 (not '*Echinomys* Desmarest, 1817').

155. *Dolichotis* Desmarest, Journ. de Phys., LXXXVIII., p. 211, March, 1819 (not '1822').

But however desirable it may be to obtain the earliest reference, a generic name can not date farther back than 1758 (the year when the 10th edition of Linnæus' Systema Naturæ was published) or before the time when it was used as a scientific and not a vernacular name. Brisson's genera of 1756 must date from 1762, and French names should not take precedence over others published later, but before the former appeared as Latin names. The following genera should therefore be quoted as indicated below:

Spermophilus Cuvier, Dents des Mamm., 1825, pp. 160–161, 255, pl. LV. (not 1822), *Glis* Brisson, Regn. Animale, ed. 2, 1762, pp. 13, 113–118 (not 1756); *Atherurus* F. Cuvier, Dict. Sci. Nat., LIX., 1829, p. 483 (not G. Cuvier, Règne Animal, 1829); *Cercolabes*, Brandt, 1835, Mem. Acad. Imp. Sci. St. Petersburg, 3d ser., III., pp. 55–58 (not F. Cuvier, 1822); *Hydrochœrus* Brisson, Reg. Animale, 1762, pp. 12, 80–81 (not 1756).

Neither should the *apparent* date of publication be accepted when there is evidence to show that the name actually appeared earlier or later than indicated by the title page of the volume in which it was printed. For this reason *Anomalurus* should date from January, 1843, not 1842; *Psammomys* 1828, not 1826; *Oreinomys* 1881, not 1880; *Saccostomus* 1846, not 1847; *Acomys* 1838, not 1840; *Chiropodomys* 1868, not 1869; *Zapus* 1875, not 1873; *Pectinator* 1856, not 1855; *Schizodon* March, 1842, not 1841; *Chætomys* 1843, not 1848; *Lagostomus* 1829, not 1828. This question of exact dates may seem a very trivial matter, but when a difference of only a year or two in publication has necessitated the rejection of such well known names as *Arvicola*, *Isomys* and *Ochetodon*, it can readily be seen that, unless the date of publication is fixed with precision, generic names will never be stable.

* To these may be added *Fiber*, omitted from the list, and *Tachyoryctes*, which has no reference.

† Those who agree with Sherborn in not recognizing names on plates must quote *Dipus* from Boddaert's Elenchus Animalium, 1785, p. 47. In either case the authority is not Gmelin, as given by Thomas.

In two minor points the list is fairly open to criticism, namely, in the abbreviation of authorities and references, and in the emendation of names. Even those familiar with the literature will find difficulty in recognizing Ogilby in 'Og.,' Brants in 'Bts.,' Hemprich and Ehrenberg in 'H. & E.,' or in telling whether 'Sm.' stands for Smith or Smuts. In most cases Mr. Thomas has followed the original spelling of a name, but apparently with some hesitation, for he finds it necessary to apologize for *Aplodontia*, stating that he looks 'with loathing on these h-less names.' He has, however, adopted the emended forms *Echinomys* for *Echimys*, *Cannabateomys* for *Kannabateomys*, *Pithecochirus* for *Pithecheir*, and *Acodon* for *Akodon*, although in a paper subsequently published he has reverted to the original spelling, *Akodon*.

There is opportunity for much divergence of opinion as to the sequence and relative rank of the groups, for example, as to the wisdom of reducing the Lophiomyidæ to a subfamily of the Muridæ, while giving *Pedetes* and the American Porcupines full family rank. Some may question the removal of the Bathergineæ from the Spalacidæ to form a separate family placed after the Geomyidæ and Heteromyidæ, so that the Old World genera *Spalax* and *Bathyergus*, which were formerly arranged side by side, are now separated by two families of New World pouched gophers and pocket mice: Possibly, it may seem that the author has recognized a relatively large number of genera of Muridæ, in view of the statement that all the recently proposed genera of Geomyidæ "may be most conveniently treated as of subgeneric rather than generic rank, sound as their basis as natural groups no doubt is."

But whatever difference there may be in regard to minor points, the fact remains that this paper admirably fulfills its purpose as a check list of genera of Rodents. We may venture to hope that the field having now been cleared to a certain extent of nomenclatural difficulties, Mr. Thomas will soon undertake the work which has so long been needed, namely, a complete catalogue of the Rodentia.

T. S. PALMER.

WASHINGTON, D. C.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES—BIOLOGICAL SECTION, APRIL 5.

PROFESSOR OSBORN moved that a committee be appointed to consider and take action on the question of postage on natural history specimens. The chair appointed Doctors Dyar and Dean and Professor Stratford. Professor Bristol offered his resignation as Secretary. It was accepted, and the election of his successor was laid over until the next meeting.

Professor Osborn reported upon the phylogeny of the early Eocene Titanotheres, showing that they are divided into two distinct series, included under the genera *Telmatotherium* and *Palæosyops*, both of which independently acquired horns. The Telmatothere line begins with *T. boreale*, a form which Cope referred to as Palæosyops. It is distinguished by animals with long narrow skulls and high stilted feet, and undoubtedly represented the upland types of the family. The Palæosyops line, as suggested by Earle and Hatcher, passes through *P. laticeps* and *P. manteoceras*, and leads up to Diplacodon, the larger species of which surpass in size the smaller Titanotheres of the Oligocene. The main line gives off several collaterals, such as *P. paludosus*. Lambdotherium does not belong in the Titanothere phylum at all.

A second note related to a division of the two groups of placental mammals, the Meseutheria and Ceneutheria. The former, since Wortman's demonstration that the Ganodonta are ancestral Edentates, must now embrace this division, besides the Creodonta, Lemuroidea, Tillodontia, Insectivora, Amblypoda and Condylarthra.

The third note related to the origin of the typical mammalian types of teeth among the Theriodonta, Cynodontia and Gomphodontia of the Triassic. It is especially noteworthy that the Gomphodontia afford a demonstration of the origin of multituberculate teeth from a tritubercculate ground plan, as hypothetically assumed by the speaker some years ago.

Mr. Bradney B. Griffin reported that in Thalassema (one of the Echiurids) the spireme occurs in minute ova (3 micra in diameter) floating in clusters in the body cavity. The spireme segments into one-half the somatic

number of chromosomes, which by partial longitudinal splitting pass into flattened ellipses. These elongate, and during the growth period become twisted and distorted, and their true shape is thereby obscured. While entering the first polar spindle they appear as loose open rings or compact rods (bivalent). These by concentration and looping up form crosses, opposite arms of which are attached to the 'Zugfasern.' During metaphase the crosses become drawn ont into flattened ellipses which split across into two V's with closely apposed limbs. At telophase the latter separate at the angle and diverge in the second polar mitosis. No longitudinal splitting of the V's occurs.

In Zirphæa (Lamellibranch) the process is identical, although more obvious by reason of the less close apposition of the halves of the rings and V's. The conclusion is that in both forms a reducing division takes place.

Mr. J. H. McGregor offered a preliminary report on the development of the Spermatozoa in Amphiuma. Professor F. E. Lloyd's paper on Pholadidæa of the Pacific Coast was read by title.

C. L. BRISTOL,
Secretary.

MAY 3, 1897.

Mr. Gary N. Calkins, of Columbia University, was elected Secretary of the Section.

In the absence of Dr. Dyar, chairman of the committee appointed to consider the question of postage on natural history specimens, Professor Stratford reported that the Postmaster-General had been notified, and that the matter had received due consideration.

Upon behalf of the committee appointed to draw up a resolution relating to the death of Professor Cope, Professor Osborn delivered a brief eulogy of the great naturalist, pointing out the especial features which have made his work famous and have given him such a high position in the history of natural science. He dwelt especially upon the fact that Professor Cope prosecuted five great lines of work simultaneously, and that in each he acquired a commanding position. He also spoke of some of his generous qualities as a fellow scientific worker, especially his liberality in the loan of collections and generous recognition of the work of others. Finally, he alluded to his remarkable independence and fortitude of character, and persistent devotion to science, even with limited resources. His death leaves a vacuum especially in the line of able and accurate criticism of contemporary work. Professor Osborn concluded by submitting the following resolution:

The members of the New York Academy of Sciences desire to record their admiration of the noble services to science of the late Professor Edward D. Cope. Since 1895, when he offered his first contribution to the Philadelphia Academy of Sciences, at the age of nineteen, he has been a devoted and brilliant investigator in five great branches of natural history—ichthyology, herpetology of the batrachians and reptiles, mammalian paleontology, historical geology and philosophy. In each he has long been an acknowledged leader, and his combined knowledge of all has given his researches a philosophical breadth, grasp and permanence which place him among the great masters of comparative anatomy—Cuvier, Owen and Huxley. We deeply regret that his untimely death has cut short his life work, and feel that the loss of his keen, critical and productive faculty deals a blow to the cause of comparative anatomy of the vertebrata throughout the world, which can hardly be measured. We tender to the American Philosophical Society and to the Academy of Natural Sciences of Philadelphia, of which Professor Cope was a life-long member, an expression of our deep regret at their loss, and of our readiness to cooperate with them in the establishment of some suitable memorial.

Signed: HENRY F. OSBORN.
J. L. WORTMAN.

Mr. H. E. Crampton, Jr., gave a brief abstract of a paper by F. C. Baker on 'Notes on Variations in the apex of Gasteropod Molluscs.' Professor Bashford Dean and Mr. F. P. Sumner reported on the spawning habits of *Petromyzon Wilderi* at Van Cortlandt Pond. Mr. H. E. Crampton, Jr., reported on some Coalescence Experiments with Lepidoptera. A paper on the 'Vertical Distribution of Plankton in Deep-Sea Collections from Puget Sound,' by Professor James I. Peck and N. R. Harrington, was read by title.

G. N. CALKINS,
Secretary.